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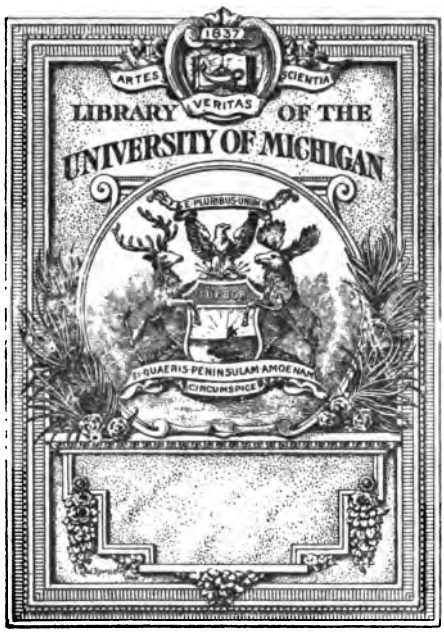
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# THE MONTHLY MICROSCOPICAL JOURNAL:

34506

## TRANSACTIONS

OF THE

## ROYAL MICROSCOPICAL SOCIETY,

AND

## RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

HENRY LAWSON, M.D., M.R.C.P., F.R.M.S.,

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VOLUME XI.

LONDON:

ROBERT HARDWICKE, 192, PICCADILLY, W.

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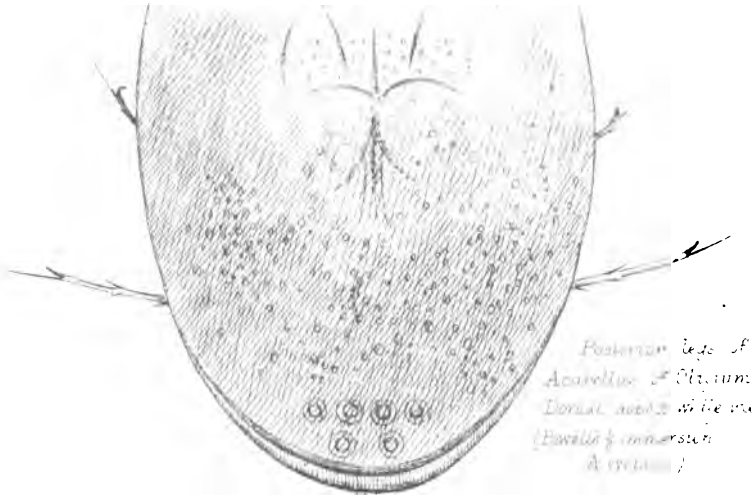
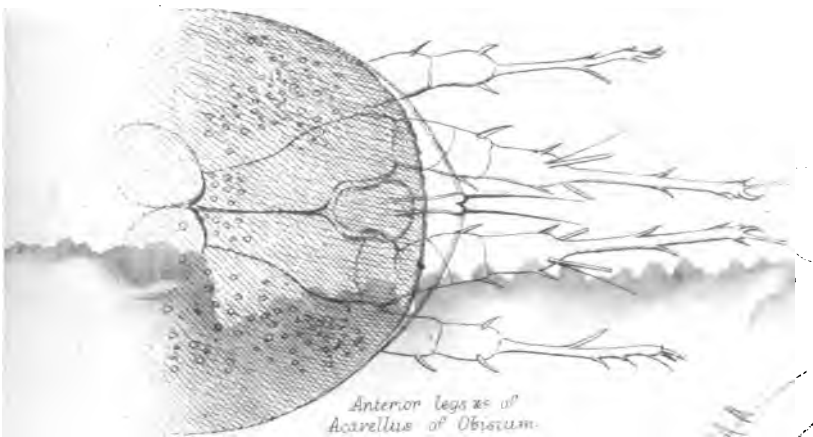


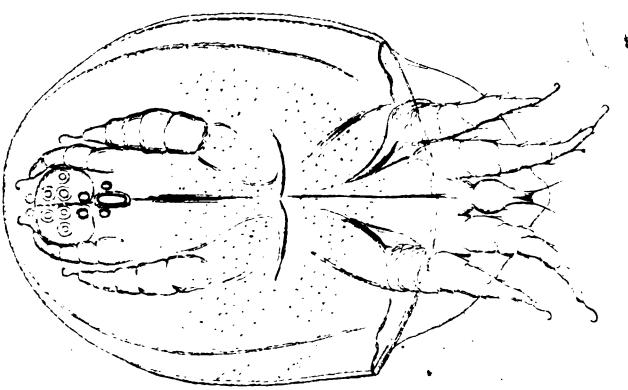
Posterior legs of Acarellus of Obisium. Dorsal aspect while resting (Parelli ? immersion A 1 eyepiece)



Anterior legs of Acarellus of Obisium.

Dorsal aspect while at rest

Copy of rough sketch of Ecdysis of Acarellus Pulicis — Drawn immediately after mounting in balsam.



*Acarellus Pulicis*, also found on Chelifers ×180

THE

# MONTHLY MICROSCOPICAL JOURNAL.

JANUARY 1, 1874.

## I.—*Notes on so-called Acarellus.* By S. J. McIntire, F.R.M.S.

(*Read before the* Royal Microscopical Society, *Nov.* 5, 1873.)

Plate XLV.

Whilst watching the habits of Poduræ and Pseudo-scorpions I have often been conscious of the presence in the cork cells of uninvited guests in the shape of acari of various species, either collected unintentionally by the camel-hair brush into the test tube together with the desired captures, or migrating from one cork cell to another of their own free will and pleasure. One of these has engaged a good deal of my attention.

The first time I noticed it was in its character as a parasite upon a rather fine specimen of Obisium; six or seven of them clinging firmly to the legs and cephalothorax of the host, and remaining in the position they had taken up for days without causing any apparent inconvenience to the obisium. Indeed, at first I scarcely recognized them as parasites. Afterwards it was by no means uncommon in the locality which was my happy hunting ground (a back-yard with an apology for a garden at the rear of our house) to find under old bits of wood, brickbats, &c., Gamasi and Obisia, infested with these parasites, and once I caught a Gamasus with a perfect load of thirty or so of them on its back and legs, rendering it quite unrecognizable at first sight. By this time I had come to the conclusion that the earth in this particular place swarmed with this acarus, and it was no longer a novelty. I felt quite certain, moreover, that there were two species.

On transferring the infested Obisia, &c., to my cells, I soon found that their little parasites occasionally left the host's back and wandered over the floor and sides of the cork cells; sometimes fixing themselves on the glass cover. Whilst in this position I availed myself of the opportunity of a closer inspection, and occasionally mounted them in balsam.

Up to this point I had not been able to guess what they were, but now I obtained some scanty information. I saw that after they were mounted the two hinder pairs of legs were not so apparent as when the creature was alive. In most cases the position

these legs took up and the refractive character of the medium rendered them nearly undiscoverable, and in this state I afterwards recognized one of them as the Hypopus of the 'Micrographic Dictionary,' and as identical with Topping's preparation of the Parasite of a House-Fly,* which he always designates as "rare." I read the description in the 'Micrographic Dictionary,' under the heading Hypopus, and had no reason at that time to question the suggestion of Dujardin there alluded to, about the origin of these said Hypopi. In my article in 'Science Gossip' for 1869, p. 243, on Pseudo-scorpions, I mentioned both these parasites, and furnished rough figures of them, but owing to some misunderstanding only one of the illustrations was inserted; the other was omitted from the paper altogether.

About this time also I noticed that my cells were infested with another acarus,† very like a cheese-mite, but distinguishable on account of its dirtier appearance, especially in its earlier stages. A group of half a dozen of them would be seen devouring the decaying malt and other substances—in fact, wallowing in the filthy mess they made like so many tiny pigs; and in some cells all the corners where the pabulum of the Poduræ or their excrement had accumulated were occupied by similar groups of these disagreeable-looking acari. I desired greatly to expel the dirty intruders, but as it generally happened after making a collection of Poduræ, &c., that a few fresh ones were inadvertently introduced, I concluded that the task was hopeless, for the reason that they swarmed in the earth equally with the Hypopi previously mentioned; and so I paid them no more attention.

Two years ago, however, in the month of September, I picked up a decayed potato, which had such a large population of these acari upon it that I was induced to give it some close scrutiny, chiefly with the view to satisfy myself whether these mites were the so-called "cheese-mites" (*Acarus domesticus*) or another species. I soon saw that there was a remarkable change going on in the case of the greater number of them. They were casting their skins; and when this operation was complete they had metamorphosed into my other little friends, the Hypopi.‡ So curious an incident prompted me at once to sweep off all the acari into benzole and mount them in balsam. For some weeks afterwards the slides thus made showed the interrupted stages of the process of ecdysis very well, but time has altered all that now. The slides (or rather, the only one I have left at the present time) exhibits the Hypopi more

* I speak from memory only, and may be wrong; anyhow it is a very closely-allied species.

† I have since come to the conclusion that these also may be distinguished as *two* species, one of them much more like a cheese-mite than the other.

‡ I am disposed to consider this Hypopus identical with Mr. Tatem's *Acarellus Pulicis*, to be referred to presently.

or less distinctly, but the cast skins and the specimens in an early stage of the process have shrivelled up beyond the most scanty recognition.

I did not then call attention to the curious incident, beyond exhibiting the slides at one of the Wednesday evening conversational meetings of this Society, and it had nearly passed from my memory till I read a short paper by Mr. Tatem, in the 'Monthly Microscopical Journal,' on some new Acarelli. In that paper two species are figured, and described as *A. Muscæ* and *A. Pulicis*: the former strongly calling to my mind Topping's "rare" parasite of the house-fly, and the Hypopus of the 'Micrographic Dictionary,' while the other, which Mr. Tatem says he found in a dead flea, is, I believe, the same species which I detected in the act of ecdysis, recorded here, and which I am well acquainted with, as parasitic upon various arachnida. As Mr. Tatem says his figures were from the balsam preparations, I could at once understand why the curious rostrum is omitted, and the two hind pairs of legs are figured as they are in the illustration. Had Mr. Tatem seen them in the living state under the microscope suitably illuminated, I feel sure his figures would have been different, and he would have modified what he says in the first paragraph of his communication. For certain all the legs are *free*, and the statement that the two posterior pairs are "neatly packed up in their trunks ready for evolution in the progress of growth" is not correct. The Hypopus, or Acarellus, walks upon all its eight legs. The two posterior pairs are very short, but each of them is furnished with a very long and delicate bristle (only seen well in life), which materially assists the creature in its small powers of locomotion.*

With regard to the other important statement about their "obviously imperfect development," I must also venture to differ from him, from the incident I have above recorded.

I communicated with Mr. Tatem on the subject, and forwarded him slides containing specimens, some of which he recognized as very like his own Acarellus, and others were not so conclusive. I also sent him cork cells containing living examples of the Acarellus in question, and its suspected earlier stages, but ill-health at the time prevented his giving the necessary attention to the subject, and so he returned me the cells with the request that I should pursue the inquiry, courteously admitting (if my recollection is correct) that there was the possibility of a mistake in his conclusions, as the data he had to go upon were so scanty, and the balsam preparations and previous liquor potassæ treatment might have obliterated the view of certain important points. I have to thank

* This statement has reference more particularly to my supposed *Hypopus* (or *Acarellus*) *Muscæ*. The other Hypopus has shorter legs, terminating in single claws.

him for his kind communications, and the courteous manner in which he met my contradictions.

Accordingly I have kept the two cork cells under occasional observation, and though I have not been fortunate enough to meet with another case of ecdysis, I do find that the cell which was tenanted with the cheese-mite-looking acarus I have alluded to has now numerous examples of Hypopus (or Acarellus) in it, and the cell which contained Hypopi or Acarelli now contains many of the suspected earlier stages. Of course there is the possibility of migrations of the two creatures from their respective cells; but I incline to the view that the Acarelline form is an adult one, generally parasitic on certain of the minute Arachnida, and in its early stage or stages a vegetable feeder, and totally unlike the form it ultimately assumes, being, moreover, in this early stage, fully twice the size of the adult form.

I doubt very much the suggestion both in Mr. Tatem's paper and in the 'Micrographic Dictionary' (under the authority of Dujardin), that the Hypopus or Acarellus is one of the early forms of Gamasus; for my cells are very much infested with these creatures. To the best of my belief their young are white and very active. The structure of the mouth also in my opinion indicates a wide difference.

The active little mites which I think are the larvæ of the species of Gamasus occurring in my cells, feed chiefly upon the same food as the Poduræ (crushed malt); but the adults will attack and devour the young Poduræ. I have also seen them seize and carry off, after a struggle, the end joint of the antenna of a full-grown one,—the antenna having been introduced into the hiding-place of the Gamasus by its unsuspecting owner bent on exploration. In the open space these Gamasi cannot cope with the superior strength of the adult Podura, but when one of them dies from old age, the Gamasi appear on the scene in force and soon clear away the corpse.

On the other hand, the mite, which in my opinion is the larval form of Hypopus, is very sluggish, and seems most at home when absolutely immersed in filth, resulting from the decay of the excrement of the Poduræ, &c., and the fungoid growths arising in the cells. I am sorry I cannot figure this creature, as I have never been able to see it under a higher power than a two-thirds and opaque illumination, except at the certain risk of the escape of all the other inhabitants of the cell, and these have been so interesting that I could not entertain the idea of disturbing them. The figures I have given of the *Hypopus Muscæ* are from life. I inverted the cover of the selected cell on which I saw several of the Hypopi had settled, and endeavoured by means of water and a fine camel-hair brush to turn them on their backs. But though they might be walking, immediately the brush touched them they clung to the glass by means of the suckers at the posterior extremity so firmly,

that I could not dislodge them except in a rotary fashion, and I was obliged at last to cover with a thin piece of glass and draw them from the dorsal aspect under Powell's $\frac{1}{8}$th (immersion front).

It is with some hesitation I record these notes in opposition to the opinions of such accurate observers as Dujardin, Professor Westwood, and Mr. Tatem. It is only from reading Mr. Tatem's paper that I gather that his Acarelline forms are similar to Professor Westwood's, and in both the examples he gives I doubt the four-legged characteristics. Also, the prominence of what seem to me to be the reproductive organs conveys to my mind the notion that the Acarellus or Hypopus is an adult, not a larval, form. Having been unable, however, to see these parts in life to my satisfaction, I cannot make a drawing of them; and the structure of the tubular mouth, with the two terminal setæ, prevents my associating it in any degree with the nippers of Gamasus. In one mounted specimen of Hypopus, I observed lately a curious distension of the end of the rostrum, reminding one of the lips of the blow-fly, as if this organ were used for suction.

Perhaps these notes may induce others to communicate facts in their experience in support or otherwise of my theory, and this shall be my apology for intruding my remarks on the subject.

When drawing the living *Hypopus* (*Muscæ*) under the microscope ($\frac{1}{8}$th objective), I could not help noticing the beauty of the anterior pair of legs, and the remarkable spoon-shaped tenent hair at the extremities. Its delicacy would I think quite render this hair (or pulvillus) invisible in a mounted specimen. I could only see one claw on each foot, but from the position of that member in the anterior pair of legs, desperately holding on to the glass by means of the tenent hair, a second claw might have been hidden from view.

Having seen Mr. Tatem's slides within the last few days, I have altered my opinions as to the identity of the respective creatures somewhat. His *A. Muscæ* is different from mine, and is probably a new species. His *A. Pulicis* confirms me in the suspicion expressed in a preceding note that it is an old acquaintance of mine, and the bleached appearance of both specimens makes me think more than ever that the potash treatment has had a very injurious effect, and hidden delicate structure from view. In the present state of the specimens I willingly lend my testimony to the accuracy of Mr. Tatem's figures of them.

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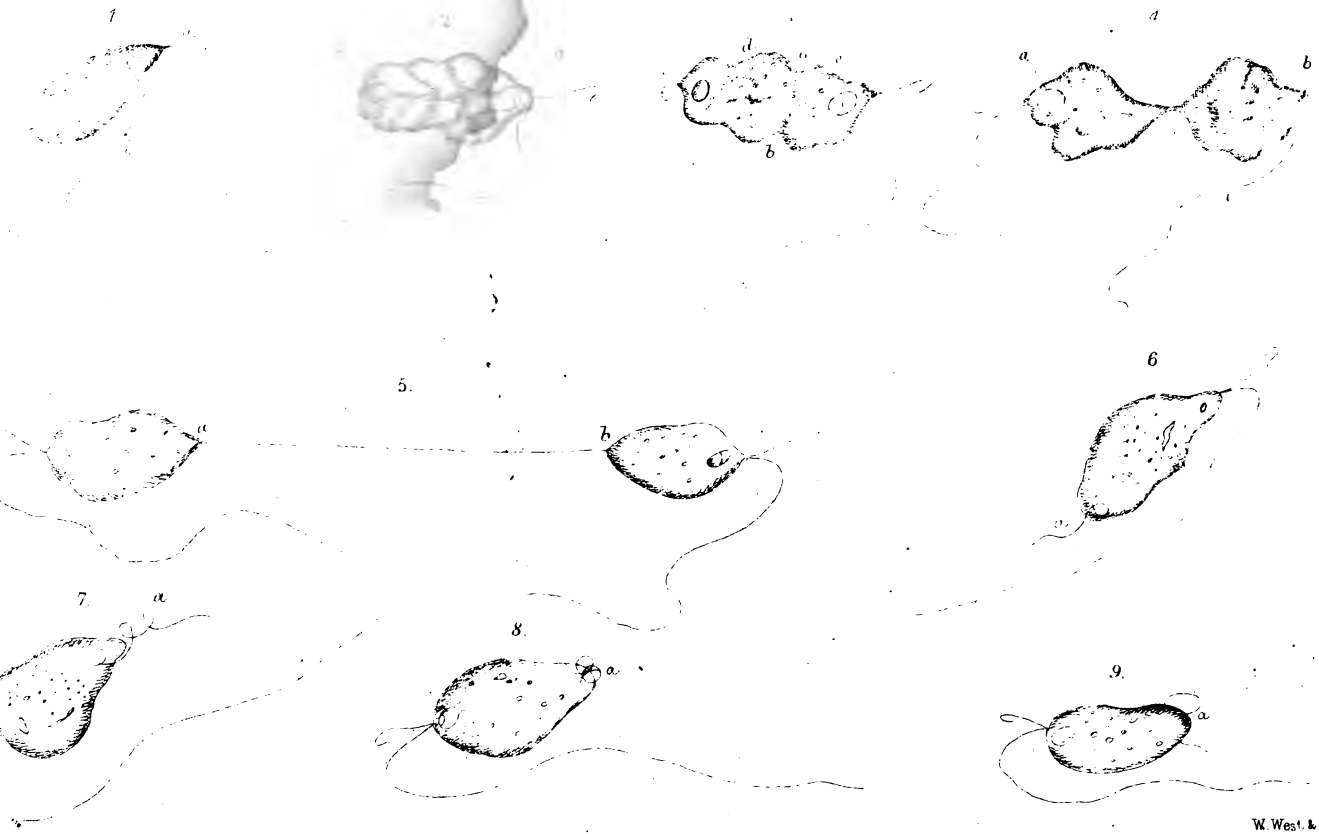
### *Additional Note.*

*November* 13, 1873.

I find, on comparing notes with Dr. Gray and others, that the Acarelline form, called by Mr. Tatem *A. Muscæ*, is by no means un-

common on house-flies; and the specimens which have been shown me in illustration of the fact are identical with that kindly sent for exhibition by Mr. Tatem on Nov. 5. So that the species which I have alluded to in my paper as *A. Muscæ*, and which I at first thought was the same as Mr. Tatem's, must receive another name; but the task of giving it a name I leave to some one better qualified than myself. As I have stated in the paper, I first found it on an Obisium, and afterwards on Gamasi, and have strong reason for thinking the early stages of those now in my possession were passed in my cells in the character of a vegetable-feeding mite of very unprepossessing habits and exterior. Although in the three examples of this genus of the minute Arachnida recorded in Mr Tatem's paper and mine, I am sure that the four-legged theory is a mistake, I am not in a position to say the theory may not hold good in the case of other mites which I have not met with. It is necessary I should clearly explain myself on this point, as it appears from conversation with certain Fellows of the Society, who heard the paper read, that I have left it doubtful whether I have not been trying to upset well-authenticated facts in regard to the imperfect development of the legs in the Arthropoda, a position which I have not the least intention to assume. I have just succeeded in mounting the mite in question in balsam, and, as I expected from analogy, the hind legs quite vanish from view unless they happen to take a position in which they can be seen clear of the creature's body.

S. J. McIntire.

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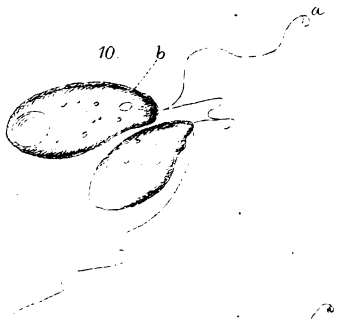
Researches in the Life History of the Monads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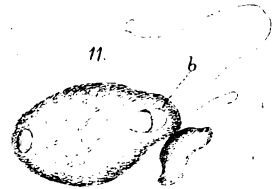
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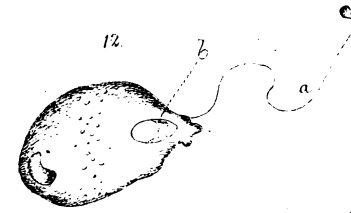
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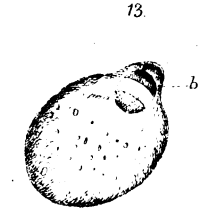
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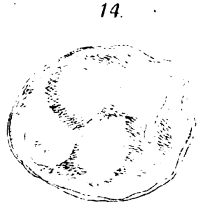
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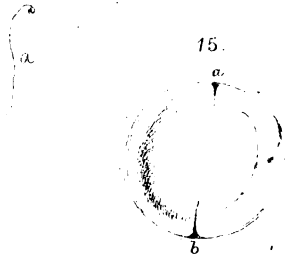
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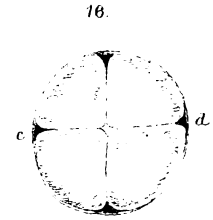
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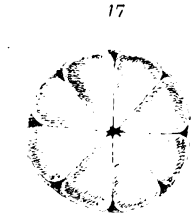
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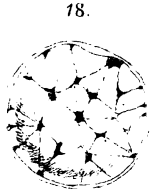
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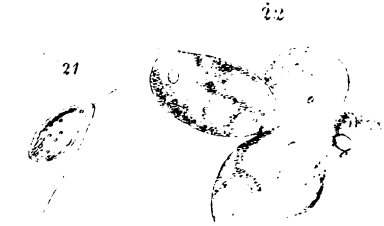
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## II.—*Further Researches into the Life History of the Monads.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.

(*Read before the* Royal Microscopical Society, *Dec.* 3, 1873.)

Plates XLVI., XLVII., and part of XLVIII.

### *No. II.*

In pursuing our researches we have become practically convinced of the importance of what we have theoretically assumed—the absolute necessity for prolonged and patient examination of the same forms. Two observers, independently of each other, examining the same monad, if their inquiries were not sufficiently prolonged, might, with the utmost truthfulness of interpretation, assert opposite modes of development. Competent optical means, careful interpretation, close observation, and *time*, are alone capable of solving the problem.

It is no matter of surprise to us that fission has been so generally accepted as the entire method of increase among the extremely minute monads. It is an accurate statement of facts, so far as they go; but in no case that has been persistently inquired into by us has it proved the *essential* method of multiplication. Nor has fission itself, in these exquisitely minute forms, been described with sufficient care. It is not a mere division of undifferentiated sarcode into two parts. Before separation takes place there is always a germination of the anatomical elements, which make the new monad complete; while in many instances the fission is preceded by a suddenly induced amœboid condition. The form we are about to describe will illustrate these points.

It is a form found in the maceration referred to in our last, when in a more advanced condition of decay. It is extremely various in size; but averages from the 3000th to the 4000th of an inch in long diameter. Its general aspect is shown in Fig. 1, Pl. XLVI., where it will be seen it is a long ovate form, pointed at one end, and possessed of two flagella, one (*a*) permanently hooked, the other (*b*) gracefully flowing in curves behind. They swim rapidly, but by a series of jerks or springs following each other in constant succession, and coincident with the movements of the hooked flagellum,

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EXPLANATION OF PLATES XLVI., XLVII., and part of XLVIII.

Figs. 1 to 4.—Earlier stages of fission in monad.
„ 5 „ 9.—The process of actual division and formation of reserved flagella.
„ 10 „ 20.—The process of genetic reproduction.
Fig. 21.—A monad genetically produced with the hooked flagellum not completely formed.
Figs. 22 to 28.—The process of genesis when more than a pair of monads copulate.

which appeared to be the sole organ of locomotion. When under the cover-glass in the moist chamber they persistently formed a ring, about a hundredth of an inch from the edge of the cover all round, leaving the middle of the cover to mere stragglers and other forms.* The object appeared to us to be to get as near as possible to the source of oxygen. Fission was as frequent and striking amongst them as in the other cases we have pointed out; but its first indication was a sluggishness of motion, and the appearance of a whitish semi-opaque spot in the sarcode under the hook, as seen in Figs. 2 and 3, *ibid.*, and at the same moment it would become squarish and amœboid all over, as seen in these figures. At the same time the spot *a*, Fig. 2, repeatedly opens from a median line nearly to its margin (revealing no internal structure), and suddenly *snaps* together again like the rapid closing of the eyelid. This is a phenomenon the nature of which has entirely defied our most careful and untiring inquiry, and it is one that has presented itself in several forms.† In his paper on "*Protococcus pluvialis*" ('Ray Soc.,' 1853), Cöhn says (p. 534) that the protoplasm "possesses the faculty of forming vacuoles at all times, and even externally to the cell; a property, it is true, which has for the most part been hitherto overlooked or misinterpreted." To which the editor, G. Busk, adds, "sometimes, as in the zoospores of volvox, these vacuoles exhibit rhythmical contraction." Whether this is a phenomenon similar to the one we describe above, our examination of volvox has not enabled us to decide, but it is a feature deserving careful and competent research.

If now a power of about 4000 diameter be used, and the light carefully manipulated, another oval spot *b*, Fig. 2, will be seen, but *without* either the opening or shutting seen in *a*. Constriction now presents itself as at *a*, *b*, Fig. 3, and this gradually increases, as seen in Fig. 4 of the same Plate, where the monad has turned round in the process of fission, but the letters *a* and *b* stand for the same parts as in Fig. 2. Before the division had proceeded further, we were in our earlier examinations surprised by the sudden appearance of the hooked and the trailing flagella on the side *b* (originally of course without it) of the form, as drawn at Fig. 4. How this came was at the time inscrutable to us. The progress of the division was now rapid, and by focussing down or up and using the light with care, we were enabled to see the stretched filament between them as drawn in Fig. 5; but when this snapped it was wholly lost to view, and we could see no trace of it again. Indeed, it was not quite clear that it did not snap off at *a* and *b*, and become wholly detached. To this question we paid close and continuous attention, and after many days of observation we were

* This is not peculiar to this form, but is more manifest than in most others.

† 'Microscopical Journal,' p. 203, Nov. 1, 1873.

enabled to demonstrate that the filament *a, b*, snapped in the middle and flew back in a coiled condition, as seen in *a, a*, Figs. 6 and 7, and in a more advanced state of occlusion at *a, a*, Figs. 8 and 9; and very shortly this delicate coil, which was repeatedly seen, although always with difficulty, became lost in the sarcode.

We now carefully watched the initial stages in fission, and were enabled to discover with the first evidence of constriction the shooting out of a short coil at the non-flagellate end as drawn at *c*, Fig. 3; this was increased by another coil and a rapid whip-like motion until the entire flagellum *c*, Fig. 4, was thrown out. Thus there appeared to be a utilization of a reserved flagellum, produced originally by fission. The mode of origin of the hook has eluded us; but it appears to arise from an extrusion of sarcode. Each half of the divided monad is thus equally differentiated before final separation.

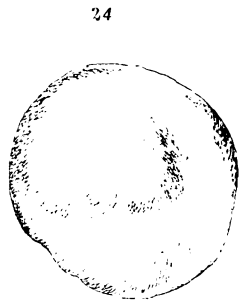
As in all the other instances we have observed, multiplication by this method continued with no apparent interruption for several days. But in our prolonged observations on the mode of fission we had repeatedly seen two of these monads uniting; that is, one would apparently fix on the body of another. On closer examination we saw that the contact was not necessarily permanent; but where it was the following facts were repeatedly seen. One monad with a sort of eye-spot, *b*, Fig. 10, Pl. XLVII., and a knot at the end of the flagellum *a*, *ibid.*, would fix on the sarcode of another without it, and the substance of the lesser or under one would be absorbed by the upper; the latter increasing palpably in bulk, as seen in Figs. 11 and 12, whilst the eye-spot *b* soon began to open and shut, and gradually to increase in size. Eventually—in about two hours—the merest trace of the lower one was left, Fig. 12: and the flagellum *a* moved sluggishly, and in another hour it had anchored as in Fig. 13, and the eye-spot *b* had reached its largest size and most constant motion. In the course of from forty minutes to four hours this ceased; all trace of the eye-spot was lost, and a yellowish gelatinous-looking flabby mass resulted, which is shown at Fig. 14, and was severed from the flagellum *a*. Distension rapidly ensued; so that a spherical form was taken by the mass; and after repeated watchings on forms in the same condition it was made out with a power of 3500 diameters, that openings at *a, b*, Fig. 15, appeared, and a faint line connected them. In less than five minutes after they appeared also at *c, d*, Fig. 16[3], with a faint line at right angles to the first; and in ten minutes after it had taken the form shown in Fig. 17[4]. From this time an internal division in all directions took place, represented in diagram rather than in actual portraiture, at Fig. 18. The activity that now displayed itself within this tiny ball can scarcely be conceived, for the whole interior sarcode in about three hours had broken up into innumerable

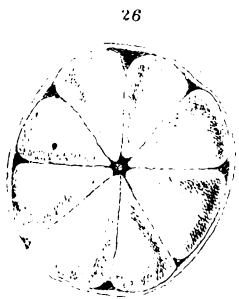
beautifully-formed oval bodies, as seen in Fig. 19. Its appearance at this stage was extremely beautiful; and subdivision being complete, the oval bodies were in constant motion within. We were quite unprepared for what ensued; this object was carefully watched for about two hours and twenty minutes, when the thin investing membrane opened, and minute oval bodies, moving freely, and in many of which a single flagellum could be seen, were set free. The aspect of this emission when nearly complete is given at Fig. 20. The growth of these emitted bodies was rapid; but we could not discover how the hooked flagellum first appeared. In some few instances it was seen as in Fig. 21, before the hook had formed upon it; but its first appearance was unperceived. These minute forms were watched through all the stages of growth until the phenomenon of fission presented itself, and the cycle was complete.

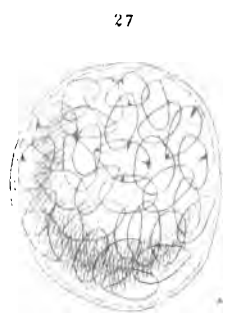
But the union of *two* monads is not the only method. A union of four, and even of six, has been on many occasions seen by us. At Fig. 22 is a drawing of the blending of *four*, as seen in an early stage. The blending in all essential respects proceeded as before, only one eye-spot being developed at *a*. At length the sac-like conditions presented themselves as at Figs. 23 and 24, Pl. XLVIII.; when division ensued as at Figs. 25 and 26; when the multiple internal segregation followed, as in the former case, reaching the condition drawn at Fig. 27, and passing to that shown at Fig. 28, when all was as before. In this case the details could be more easily made out from the increased size of the object.

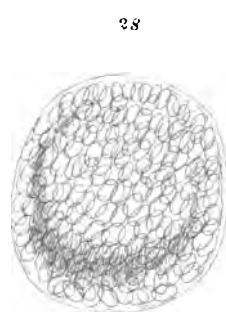
We have here, then, a life history comparatively simple. Fission is the most apparent mode of increase. This is preceded by a differentiation of the part to be divided, characteristic of the perfect monad; and by a marked amoeboid condition. By the fission a reserved flagellum for future fission is apparently formed. This may continue for many days, but eventually two, four, or even six of these monads may unite together—take a flaccid sac-like form, becoming quickly distended, and dividing internally into segments, which go on subdividing until the sac is filled with beautiful oval bodies which eventually escape, and are found to possess a single flagellum; these rapidly grow, acquiring in a manner not clearly made out the second (hooked) flagellum, and when thus mature recommence multiplication by fission.

The effects of heat on the immature state of this form differ in some striking particulars from the effects of the same temperature on the forms we have already described. The results of our experiments on temperature, with all the forms examined, will be given in our next communication.

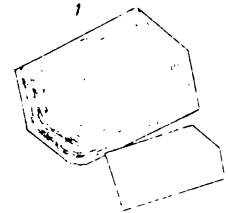


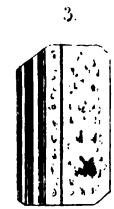



Researches in the Life History of the Monads





*The Microscopic Structure of a granitoid quartz porphyry*

## III.—*On the Microscopic Structure of a Granitoid Quartz-porphyry from Galway.*

By Professor Edward Hull, M.A., F.R.S.

Plate XLVIII. (Part of).

*Granitoid Quartz-porphyry, Attithomasreagh, Co. Galway.*—This is a finely-crystalline granular rock, of a light reddish colour, consisting of reddish and yellowish felspar, silica in distinct grains, and green mica; in which are imbedded well-formed crystals of reddish felspar. The position of this rock is near Salt Hill, about a mile south-west from Galway, and it occurs amongst the great mass of granitic and felspathic rocks which stretches westward from that town.*

If we assume that in all true granites the base is silica, enveloping the other constituents, the microscopic structure of this specimen tends to show that this rock is not a true granite but rather a quartz-porphyry, as the base is seen to be felspar, in which are enclosed all the other minerals, including the silica itself. At the same time, from its granular character and the presence of all the constituents of granite, it may be considered as bordering on the region of the granitic series.

*The Base.*—In order clearly to observe the base, a rather high power (about 100 diams.) is required. It is colourless, but that it is not "a glass" is proved by its action on polarized light, for on rotating the analyzer it exhibits a waved or twisted structure with a varied play of prismatic colours, in all probability due to imperfect crystallization. The base is therefore clearly felsitic, and as it is intimately interwoven with all the other minerals, it forms a considerable proportion of the entire mass. With a higher power (400 diams.) the crystalline structure is very apparent, and intermixed with the felsite itself may be observed much free silica containing numerous cells.

*Felspars.*—Orthoclase in numerous well-formed crystals is the most abundant constituent of this rock, but along with this there

EXPLANATION OF PLATE XLVIII. (Part of).

Fig. 1.—Twin crystals of orthoclase—the larger of which shows a faint internal structure parallel to the sides of the prism. The smaller is clouded, and exhibits no structure internally. Magnified 25 diams.

„ 2.—Orthoclase crystal formed round amorphous felsitic material. Magnified 40 diams.

„ 3.—A double crystal of triclinic felspar and orthoclase.

„ 4.—Portion of a grain of silica containing numerous cavities, some with fluid bubbles, others with solid materials (stone-cavities), others empty. Magnified 400 diams.

* See Map of the Geological Survey, sheet 105, with "Explanation."

are a few crystals of a triclinic felspar (probably oligoclase), exhibiting with polarized light the fine parallel lines characteristic of the triclinic group of felspars. The orthoclase crystals often show the apparent angle of 90°, and when small are well formed. In one or two cases a faint internal structure, parallel to the sides of the crystal, may be noticed, which I have attempted to represent in Figs. 1 and 2; but in general the interior is clouded and structureless.

*Silica.*—The grains or "blebs" of silica sometimes show a slight approach to the crystalline form, but are generally rounded and structureless.

With the 1-inch object-glass (mag. 55 diams.) the silica is seen to contain an immense number of cells just coming into view with that power. Sometimes they seem to lie in lines or rows in different directions. So numerous are these cells, that in a section with an area of one-hundredth part of a square inch there are several hundred, so that in a square inch there are several thousand within a plane bounded by the sides of the slice, which is very thin. In a round grain, or sphere, of about $\frac{1}{10}$th inch in diameter, there are probably no fewer than ten thousand cells!

With the $\frac{1}{5}$th object-glass and No. 2 eye-piece, magnifying 400 diams., nearly all the cells are seen to contain fluid bubbles; which are still better revealed with No. 4 eye-piece, magnifying 860 diams. The bubble in each case seems to occupy about one-fourth of the entire space of each cell. A constellation of these fluid cavities is shown in Fig. 3, in which, along with the fluid cells, are two large "stone cavities," containing apparently broken stony materials. One of the fluid cells, tubular in shape, is remarkable for showing three distinct little bubbles, which have probably been prevented from uniting into one by intervening impediments or irregularities in the walls of the tube itself.

*Mica.*—The mica is as usual scarred, but the crystalline form ill defined; the prevalent colour is sap green, in which are occasional black patches. Along with the mica are also a few patches of a structureless green mineral, which much resembles the "chlorite" of trap rocks, and which is undoubtedly a "secondary" mineral in them.

*Iron.*—Iron pyrites appears in a few instances in the form of specks of a rich bronze to ruby colour, translucent, but not transparent; along with these are rare instances of black grains, sometimes in groups, which I assume, with some hesitation, to be magnetite. Assuming the pyrites to be a *secondary* mineral, the quantity of iron originally in this rock must have been exceedingly small.

On the whole this rock is a fair example of a large class of quartz-porphyries in which the constituents of granite are present, but which differs from a true granite in having a felsitic instead of

a silicated base. In this case the silica has consolidated into individual sub-crystalline grains before the other minerals, whereas in all true granites the silica has been the last to consolidate. The presence of aqueous (?) vapour during the consolidation of this rock is shown by the existence of numerous fluid cavities, and is another feature in which it resembles true granites. Other quartz-porphyries which I have examined show cavities in the silica, but generally destitute of fluid bubbles.

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## IV.—*The Structure of the Scales of Lepisma Saccharina.*

By G. W. Morehouse.

For many years this test has been subjected to most careful and critical examination by the most competent observers and with the best microscopes, but, after all, the true character of its markings still remains a disputed question. These differences of opinion have evidently arisen partly from the complex nature of the markings themselves, and partly from the different conditions under which they have been seen. In this scale we have coarse ribs easily seen with a very ordinary glass, and on the other hand delicate structures severely taxing the powers of the finest objectives in existence. This fact alone is sufficient to account for the want of agreement, without accusing any person of being biassed by a theory; while those observers who think their own instruments are the best will continue to be satisfied with what they may happen to see, and shut their eyes to any advance.

As the microscope has been improved, our ideas of the structure of the Lepisma scale have been gradually modified, and who will now claim it to be "too easy for a test object"?

In the order of difficulty of resolution we have—

1. The heavy longitudinal ridges running from end to end of the scale and slightly projecting at the point.

2. Distinct ribs generally radiating from the quill, or curved parallel with the outline of the scale, and becoming faint in the centre and parts remote from the quill.

3. Transverse corrugations of the membranes.

4. Faint irregular veins branching from the diverging ridges (No. 2) generally taking a transverse direction, and, together with the corrugation, causing the spurious appearance of fine beading at their points of intersection with the ridges.

To make sure of my work on this scale I have studied it under a number of different conditions. The observations have been conducted with monochromatic sunlight; with white cloud and lamp; with central beam and oblique; with mirror, prisms, achro-

matic condenser with and without central stops, and with Wenham's paraboloid. All these methods point to the same conclusions. Following up the line of observations described by the late Richard Beck, in his most valuable contribution to our knowledge of this subject, the same results were arrived at in regard to the appearance of coarse beading, &c., *viz.* "that the interrupted appearance is produced by two sets of uninterrupted lines on different surfaces."* That the longitudinal and the oblique lines are on different sides of the scale is also plainly seen by their lying in different focal planes under a $\frac{1}{5}$th objective. And further, while examining a scale in fluid I have repeatedly observed air bubbles on one surface of it confined by the longitudinal ribs, and on the other side others bounded by the oblique ridges; and on moving the slow adjustment up and down, with the movement of the bubbles under control, they never interfere or mix with each other.† Nothing further is required to prove that these markings are actually ridges, and that they project from different surfaces of the object. The experiments of Mr. Beck settle this question.

As microscopical definition advanced the very feeble radiating lines were noticed in the spaces between the ribs, formerly thought to be smooth. In the central portion of the test these lines are parallel with the main ribbing. They in their turn were seen to be uneven and pronounced to be "beaded striæ."‡ Must this fine beading like its shadowy predecessors be also extinguished by intersecting cross lines, and so add one more to the long list of illusory appearances? To attempt to throw some light upon this question is the principal object of the present article.

In the first place, it is far from being a difficult feat to see this beading. Any first-class lens, from a $\frac{1}{8}$th upward, when properly handled, will display it or something very like it. The writer has found it an easy task with Wales' $\frac{1}{15}$th immersion, or even with a Beck $\frac{1}{8}$th and deep eye-piece. With Tolles' $\frac{1}{5}$th immersion the fine transverse structure indicated above is brought out, and it becomes at once evident that the small beads are indeed spurious like their big brothers, and for a similar reason.

The fine transverse markings seem to branch from the faint radiating ones, and have the appearance of a network of minute capillaries. Beside these there are coarser transverse waves or corrugations of the membrane. In numerous instances, air bubbles have been observed imprisoned between the heavy ribs on one or two sides, and by these corrugations on the other sides. Therefore the corrugations may safely be said to be on the same surface of the scale with the longitudinal ridges, and the branching vein-

* 'The Achromatic Microscope,' Beck, p. 50.
† See 'Micrographic Dictionary,' 2nd ed., p. 34, fig. 3, pl. xxvii.
‡ See 'M. M. Journal,' March, 1873, Pl. XI., Figs. 3 and 4.

like structure on or near the other surface. Careful focussing is corroborative of this idea, making it certain that these two details of structure lie in different planes. With monochromatic light, the delineation of this structure is eminently satisfactory, and the effect of the slightest change in focal adjustment is at once felt. When the object is a little out of focus the light is unequally refracted and broken up in passing through this complicated network of ridges and corrugations, and produces an appearance of fine molecules over the whole surface of the scale.

The coarse and the fine beads both vanishing under advancing definition, together with the behaviour of the confined bubbles of air, seems to my mind fully to demonstrate the reality of the structure above described. Often, when the corrections are not perfect, the semblance of beading can be directly traced to a seeming enlargement of points of linear intersection and branching. When the $\frac{1}{50}$th is at its best work the finer transverse markings are usually irregular both in strength and direction, but always unmistakable. They may be plainly seen on some of the smaller scales and in the central parts of the larger, and at almost as good advantage as near the edges of the easier scales. Sometimes they are continuous across several intercostal spaces, and again only extending across one, or it may be merely budding, as it were, from the ribs. It will be noticed that the "beads" as drawn by Mr. Hollich exhibit corresponding irregularities.

In conclusion, the remark of Beck on the scales of Lepidocyrtus may well be quoted—"and my own belief is that the markings upon this and all other varieties of Podura-scales are more or less elevations or corrugations upon the surface, which answers the simple purpose of giving strength to very delicate membranes."* If this idea is true of the Podura, it applies with greater force to the complicated ridges of Lepisma.

The same original structure is often modified in diverging directions so as to subserve totally distinct purposes. And as hairs are probably modified scales, and a regular gradation may be traced between them, so the connecting chain is filled up between ribs extending from end to end of a scale, through undulations and shorter ribs, to those slightly projecting, and so on to the perfect spine or secondary hair.—*The American Naturalist*, Nov. 1873.

* 'Transactions of R. M. S.,' 1862, p. 83.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*Histology of the Leaf of the Tea-plant, and Value of Potass in such Investigations.*—The parenchyma of the leaf of *Thea viridis* abounds in sphæraphides; the margin of the cells of the epidermis are alike sinuous on both sides of the leaf; only apt to appear confused on the upper surface from the adhesion of some of the rounded or oval cells of the subjacent parenchyma; on the under side there are simple unicellular hairs and oval stomata. All these points may be very easily displayed by soaking the fresh leaf in a solution of caustic potass, and often still better by boiling the part in that alkaline fluid. And, as observed by Mr. Gulliver in his paper on the short prismatic crystals in several parts of leguminous plants, and in the testa of other orders, published with a Plate in the last number of this Journal, the potass is very valuable in separating vegetable fibres, membranes, and cells; and in clearing parts so as to expose many plant-crystals, otherwise but dimly seen, as was shown experimentally by him in the leaves of tea at a late meeting of the East Kent Natural History Society, at Canterbury.

*The Various Doctrines as to the Development of Bone.*—Professor Kölliker has published a work of great importance on this subject. It appears in the 'Proceedings of the Physico-Medical Society of Wurtzburg,' and has been very ably noticed in the 'Medical Record,' by Dr. E. Klein. With regard, first, to the typical resorption of bone-tissue, Dr. Klein says that the doctrine of the normal resorption of bone-tissue by ostoclasts (myeloplaxes), in the Howship's lacunæ of resorption, brought forward by Kölliker, has been lately contradicted by Strelzoff, who found that bone-tissue, once formed, never is resorbed again, but grows interstitially. In the present paper Kölliker brings forward some new facts to meet these objections. At the diaphysal extremities of long bones, the external resorption attacks, first, the periostal portion of the bone-cortex. This being here very thin, the intracartilaginous bone therefore is soon involved in the process of resorption. Such resorption-lacunæ remain for many years in the superficial layers of the intracartilaginous bone. In transverse sections through the humerus of a human fœtus, especially if they are stained with hæmatoxylin, this is quite clearly to be seen. Such sections, if they are made through the upper extremity of the diaphysis, show laterally a distinct periostal cortex, and on its external surface an apposition of bone-substance. At the median side, however, this periostal cortex is absent altogether, and the periost is in immediate contact with intracartilaginous bone, in which the residua of the trabeculæ of the cartilaginous matrix are brought out remarkably well by hæmatoxylin—a fact first pointed out by Strelzoff himself. At these points the surface of the intracartilaginous bone contains very numerous Howship's lacunæ, and in them, as usual, osteoclasts. Transverse sections through the tibia below the condyles show exactly the same. Principally, the same was found in the tibia of a male aged fifteen years.

1. *Formation of the First Vessels in Bone, developed from Cartilage; Origin of the Osteoblasts and Osteoclasts.*—In this paragraph Kölliker confirms the assertion of Lovén, Sharpey, and especially of Gegenbaur, that the marrow, in all its constituent elements, of bones, which are preformed as cartilage, originates from the perichondrium or the periost respectively. The circumstances that led Kölliker to this conclusion are these:—(*a*) In the cartilage of the epiphyses and in the short bones the well-known processes of the perichondrium, which project into the cartilage, and which contain, besides blood-vessels, a fibrillar matrix, with spherical and spindle-shaped cells, do not develop from the cartilage (Virchow), but from the perichondrium. The cartilage itself does not become dissolved, as supposed, by the progressive growth of those processes, but is simply pushed aside. (*b*) In the diaphyses of the phalanges of the embryo of calf, sheep, pig, and man, it can be shown that after the appearance of the first thin periostal crust of true bone-substance and the first calcification of the inner cartilage, processes grow from the osteogenetic layer of the periost, which spread out gradually towards all sides, and penetrate into the cavities of the cartilage-capsules. The tissue of which these processes consist is similar to that of the perichondral processes, previously mentioned, except that it is more loose, and that it contains more spherical elements. From these latter the osteoblasts and osteoclasts (myeloplaxes) take their origin. By the growth of these processes the calcified parts of the cartilage-matrix are gradually absorbed, in which proceedings the osteoclasts play an important part. The cartilage-cells themselves do not transform into cells of the marrow. (*c*) Exactly the same takes place at the ossification-margin of the diaphysis, for here the elongated vascular processes, which gradually penetrate from the diaphysal extremity into the cartilaginous epiphysis, are also offsprings of the periostal processes. Those vascular processes are always and everywhere sharply defined from the cartilage. Osteoclasts are generally not to be met with at the terminal points of the vascular processes, but they occur in great numbers near the ossification-margin; so that they play certainly a part in the resorption of calcified cartilage-matrix, but not in the dehiscence of the cartilage-cavities.

2. *Growth of Bones in Length.*—By the well-known method of feeding very young animals with madder, Kölliker arrived in agreement with Ollier and Humphry to the following conclusions as regards the growth of bones in length:—(*a*) In long tubular bones with epiphyses on both extremities, that extremity of the diaphysis grows quicker whose epiphysis remains longer separated. (*b*) Short tubular bones, with only one epiphysis, grow quickest at the diaphysis touching that epiphysis (calcaneous, metatarsi, metacarpi, phalanges). (*c*) All free edges and apophyses of any bone show a very marked growth (crista ossis illi, tuber ischii, processus spinosi et transversi, processus xyphoideus sterni, processus styloideus ulnæ). (*d*) The same holds good with certain extremities of long bones, which are provided with a considerable layer of cartilage—*e. g.* the ribs. (*e*) Short bones, with and without epiphyses, grow pretty equally on all cartilaginous

surfaces, which are in contact with other bones (vertebral diaphyses, tarsus, carpus, sternum). (*f*) All epiphyses which touch an articulation grow most at the extremity touching the articulation. (*g*) Those parts of bones that are covered with cartilage, and are not in contact with other bones, show a rather good growth. (The edges of the vertebral epiphyses, the lateral parts of all epiphyses.) (*h*) The thickness of the cartilage, whose cells are in the act of proliferation, stands generally in relation to the energy of the growth of the bone in length. There are, however, certain exceptions (vertebral apophyses). In the last paragraph of this paper Kölliker produces a new diagram for explaining the growth of long bones.

*The Micro-spectroscope in Germany.*—This instrument has been most employed in England; indeed, in Germany it is not much used in researches on colouring-matter. Still, that it has been used, and that well, in other countries, is proved by the fine work of Dr. Gregor Kraus on chlorophyll, &c., which has been favourably reviewed in the 'Academy' by no less a person than Mr. H. C. Sorby, F.R.S. Mr. Sorby says:—"In studying this subject I have been more and more convinced of the importance of distinguishing the various constituents of complicated mixtures, and my own knowledge has to a great extent advanced in the same proportion as I have been able to discover methods which could be employed with advantage in deciding whether a coloured solution was or was not of compound nature, and in determining the character of the different constituents. I have therefore paid very particular attention to this question, and have been led to a somewhat peculiar use of bisulphide of carbon, alcohol, and water, in various proportions, in order to separate the constituents more or less perfectly, and also to the employment of what I have named *photochemical analysis*, being the use of light as a reagent to decompose some of the coloured constituents and leave others, when it is difficult, or even impossible, to separate them by chemical methods. Anyone who has not tried them would scarcely believe what can be done by the use of such simple means. The application of these methods, and the comparison of the coloured constituents of all the leading classes of plants, especially those of fungi, lichens, and algæ, growing in various conditions, have led me to find that there is a considerable number of what may be called fundamental colouring-matters, which are absent or occur in very different proportions in different kinds of plants, and also in the same kind, when growing in different circumstances, besides a still larger number of apparently unessential coloured substances, which may be present or absent without materially interfering with the healthy growth of the plant. I have given a general account of these researches in a paper recently read at the Royal Society, on 'Comparative Vegetable Chromatology,'* and need not describe them now; but the results derived from these methods lead me to differ from the author in some important and fundamental particulars. In some cases preparations which he looks upon as different substances are in my opinion the same, only in one instance mixed

* The number of the 'Proceedings' in which this paper appears has not yet been published—we shall notice it when it has.

with one, and in another instance with another colouring-matter, whilst in other cases he attributes certain variable spectra to the same single substance, modified by some unknown and, as I believe, altogether imaginary cause, whereas it can easily be shown that all these variations are due to variable mixtures of substances well known in an approximately pure state, having perfectly definite and constant properties. For example, he describes and figures the spectra of the blue-green colouring-matters from *Deutzia scabra*, and from *Oscillatoria*, and shows that they differ in constant and important particulars. He hence concludes that they are two distinct substances, without any simple and definite connection, whereas by employing the methods I have alluded to it may be most conclusively proved that what I have called blue chlorophyll is the principal constituent of both, but that it is mixed in the one case with a substance found in all green leaves, though not in *Oscillatoriæ*, and in the other case with another, occurring in large quantity in *Oscillatoriæ* grown in bright light, though in relatively very small quantity in green leaves, and even in *Oscillatoriæ* when grown in a very shady place. It is chiefly in the case of the colouring-matters belonging to what I have called the xanthophyll group that the author attributes the variation in the spectra to some unknown cause, and I look upon it as very important to be able to show that this variation is simply due to a variable mixture of two or more substances, for it would lead to loose and inaccurate observations if we were to suppose that the optical characters of any separate compound could vary when dissolved in the same liquid. On the contrary, now that it can be shown by various methods that the solutions giving these variable spectra are mixtures of substances that can often be separated, and that the results can be easily imitated by artificial mixtures, there is no kind of reason for supposing that in like circumstances the optical characters of any of the separate constituents are variable. Not only is it important to establish this fact, but by distinguishing the different constituents, and determining their relative quantity in different cases, we have a perfectly simple and intelligible method of comparison, which otherwise would not be the case. The value of such principles in studying comparative vegetable chromatology will be seen at once, since it enables us to understand the exact connection and difference between the coloured constituents of different classes of plants. With such exceptions as these, which are to be attributed in great measure to the application of new methods of research, I must express my high opinion of the merits of the work, and I trust that its publication will be the means of leading to the more complete and accurate study of a branch of research which will probably yield most valuable results; only, as I believe, these will be derived, not from the discovery of rare and exceptional colouring-matters, but from the careful and accurate qualitative and comparative quantitative analysis of complicated mixtures of the most common and fundamental, which may not have attractive properties, but yet probably play an important part in the economy of particular classes of plants. When we thus study the subject, and do not ignore what might be looked upon as insignificant details, it seems possible to

draw a number of important conclusions, and to examine some of the most fundamental questions of biology from a new and independent point of view."

*Lecidea or Lecanora Ralfsii?*—This would appear to be a question which, though requiring some investigation, has nevertheless been decided. The Rev. J. M. Crombie appears to have hit upon the proper solution of the question. He says, in 'Grevillea,' that in the 'Annals of the Penzance Nat. Hist. Society,' 1853, II., p. 154, there occurs the following notice of a supposed new species:—No. 34. *Lecidea, nova species*, gathered with Mr. Ralfs at Lamorna. "This is hitherto a unique specimen, though I hope Mr. Ralfs will be able to find more of it. It consists of a thin, closely-pressed, crustaceous thallus, of a dusky-green colour, with irregular warty protuberances and flattened scales intermixed. The apothecia, which are extremely minute, have scarcely any border, and are of a dull reddish-brown. Some of them are of a dull fawn-colour; but this appears to be an older state, in which the disk has been worn away, leaving the pale colour of the apothecium visible. Should it prove to be, as I believe it, undescribed, I would venture to call it *Lecidea Ralfsii*, from its discoverer. The plant so named provisionally does not appear, at least under the proposed name, in any subsequent list of British lichens. Its identification is therefore a matter not simply of curiosity, but of importance. Did Salwey rightly conjecture that it was a new species, or is the name proposed merely another synonyme of one previously described? From authentic information recently obtained from Mr. Wm. Curnow, of Penzance, I believe that I am now in a position fully to identify this plant, and, as will be seen from what follows, it has a rather singular and interesting history. Several months ago I received from the above gentleman two specimens of *Lecidea Muddii*, Salw., to my great delight, as no British lichenist, save Messrs. Mudd and Salwey, would seem ever to have seen this lichen, nor does it appear amongst the large collection of British lichens from the latter gentleman in the herbarium of the British Museum. On first examination the specimens thus received seemed to agree sufficiently well in all respects with that plant as described in 'Mudd Man.,' p. 178, *sub Biatorina*, and I took it for granted that they undoubtedly belonged to the desiderated *Lecidea Muddii*, Salw. ('Mudd Man.,' *l. c.*), Cromb. Enum., p. 74, Leight. Lich. Fl., p. 315. The receipt, however, of several other specimens, with the apothecia in various stages of development, led me to hesitate somewhat as to the identity. This arose from the circumstance that one or two of the younger apothecia had a distinct though evanescent *thalline* margin. A more accurate microscopical examination revealed also that the hypothecium was *moderate* rather than *thin*, and *nearly colourless* rather than *pale brown*, as Mr. Mudd describes it—a discrepancy, however, which can easily be otherwise accounted for, as the apothecia in the specimen examined by Mr. Mudd were most probably old ones. On sending a specimen to Dr. Nylander for his opinion, he wrote in reply that the plant was a true *Lecanora*, and that if not the veritable *Lecidea Muddii*, it was

certainly a new species. This led to further correspondence with Mr. Curnow, the result of which was the conclusion that *Lecidea Ralfsii* and *Lecidea Muddii* were one and the same plant. The evidence for their specific identity appears to be in all respects perfectly satisfactory, and is to the following effect:—Amongst some forty specimens of the lichens described by Mr. Salwey in the above paper, one of his *Lecidea Ralfsii* was deposited in the Penzance Museum. This was borrowed by Mr. Mudd at the time when he was preparing his manual, and by some oversight or other was not afterwards returned. The identity, however, even in the absence of the original specimen, can otherwise be sufficiently established. That the original specimen of *L. Ralfsii* was identical with the specimens received by me from Mr. Curnow, *s. n. L. Muddii*, is proved by others subsequently gathered by Mr. Ralfs in company with Mr. Curnow, in the same spot, where the type, the appearance of which was quite familiar to Mr. Ralfs, was obtained. And that Mr. Curnow's specimens were identical with *L. Muddii* of Mr. Mudd's manual, is proved by their equally corresponding with the description there given of this species, except in the two minor characters above mentioned, and also in the thalline margin of the apothecia, which evidently was wanting in the single specimen seen by Mr. Mudd. It is therefore, I think, QUITE clear that *Lecidea Muddii*, Salwey, in litt. 1860, is equivalent to *Lecidea Ralfsii*, Salwey, in 'Ann. Nat. Hist. Soc.,' Penzance, 1853, and that as the latter was the first published name, the plant, for the reasons assigned, must henceforth be known as *Lecanora Ralfsii* (Salw.), Cromb."

*Tinea sycosis.*—The 'Lancet' some time ago gave a paper, by Dr. Tilbury Fox, on this subject. The paper is illustrated by two woodcuts, which show very fully the extent to which this fungus invades the hair. Dr. Fox in his lecture says:—On placing certain of these hairs under the microscope, the fungi are seen—and you can judge for yourself from the specimens I exhibit, and from hairs you take from the man's chin—to be both loaded with and ensheathed by fungus (*Microsporon mentagrophytes*), especially in the mycelial form, which radiates through and about the shaft of the hairs in the most luxuriant manner. The appearances seen on microscopic examination prove most incontestably that there is a parasitic sycosis. In some cases the fungus has not attacked the interior of the hair-shaft; in fact, sufficient time has not elapsed; but the fungus is seen in luxuriant growth about the roots of the hairs. It has broken up the connection of the hairs, its sheaths, and the follicular wall, and hence the hairs come away very readily. But you will not fail to notice certain outlying parts, that look like collections of two or three closely-packed, huge, indolent-looking acne indurata spots. This has its bearing on the diagnosis of the disease. Just take the forceps and pull at some of the hairs for yourselves, and you will see that they are lying quite loose in the follicles. You can examine them at your leisure, and compare them with the specimens I have shown you under the microscope. But the interest of the case is by no means exhausted in the recitation of these particulars; for

the man suffered at the same time from ordinary tinea circinata of the forearms, as you will see for yourselves. On the left arm is a small red patch, the size of a split pea, covered by minute scales. There are two larger ones on the right arm, on its posterior aspect just above the wrist, of the same kind, and in the scales abundant mycelial threads may be discovered.

*Action of Quinine on the White Corpuscles of the Blood.*—Herr Binz says that the power of quinine to lessen oxidation is only due to its action on hæmoglobin; and it has the same effect upon a solution of this substance as when blood itself is employed. Some years ago, Binz showed that quinine arrests the motions of the white blood corpuscles; and this effect is now explained by the diminution in the oxidizing power of the red corpuscles which the drug produces. The white corpuscles are only active when they are supplied with oxygen, and their movements are arrested by want of it. On this account, they can only crawl through the walls of the blood-vessels when oxygen is supplied to them by the red ones as they pass by. When no red corpuscles are present, Binz has found that the white ones cease to wander altogether; and this observation has also been made by Heller and Zahn. In regard to the therapeutic application of quinine, every one must judge for himself; but so much is certain, that it paralyzes the movements of the white blood corpuscles, and lessens oxidation; and it is therefore likely to be useful in suppuration and fever.—*Medical Record.*

*The Basidia of Agarics.*—A very capital paper on this important subject is that of M. J. de Seynes, in a late number of 'Grevillea.' He says that the basidia are cells which vary within sufficiently restricted limits; they are in general widened towards the summit, and more or less swollen or slender, rarely of an equal size from the base to the summit. Upon the hymenium of *Agaricus cernuus* we have seen basidia slightly compressed at the centre to take a biventral form, but this form is rare. The basidia contain a granular liquid charged with little drops of oil, sometimes slightly coloured; this liquid passes through the sterigmata or sporophores, little organs ordinarily four in number, superposed upon the basidia, and from the summit of which the spores originate. It is a sort of hollow funiculus, varying in length, sometimes slender, sometimes wide and funnel-shaped, and joined to the basidium by the wide part, sometimes describing a curve in the form of an ox's horn. During the early stage of the spore it is seen, as well as the sterigmata, to be filled with the granulations which were accumulated in the basidium. According to Corda, each sterigmata always develops one spore at a time, and sometimes one after another; although direct observation has not yet demonstrated this fact to me, it seems to be very probable, for we see the old basidia, which have employed their granulous contents in the fabrication of spores, present nothing in their interior but a clear and transparent liquid.

"When a basidium bearing ripe spores ready to be detached is found still filled with the granulous plasma intended for the spores, it is to be presumed that it will serve for a second formation, the existing

spores being entirely closed, and maintaining only a very feeble connection of endosmose with the sterigmata. We see, besides, some basidia, the plasma of which has been partly used, keep only three-fourths or a half of their cavity filled with granulations, as I have observed, and figured in a section of the gills of *Agaricus murinus;* this diminution of the contents has very likely a connection with the number of spores formed. If we were able to assure ourselves that amongst the tetraspored basidia there are but two generations, each of four spores, that would show another affinity with the thecæ of the *Ascomycetes*, which produce, as is known, for the most part eight spores. There is between the theca and the basidium such an analogue as to the terminal situation of the vegetable axis and to the production of liquid, granulous, oily contents, that we cannot but compare them completely, despite the differences in size and even of form, with products which are called upon to fulfil the same physiological function."

*Bacteria in the Blood.*—It is now pretty generally agreed that bacteria are almost invariably present in the blood; therefore the following record is not so surprising as it might have been a few years ago. It seems that Dr. Eberth states (in 'Centralblatt,' No. 20, 1873) that he has found in ordinary sweat, as well as in yellow sweat, small oval-shaped bacteria, which are frequently united in strings of two or three, and endowed with rather active movements. In spots covered with hair they attach to the hair, and often form thick layers, whilst others penetrate into the hair, which then splits and breaks. Colouring by means of hæmatoxylin brings out the isolated bacteria as well as those collected on the hair. The author thinks that they very likely contribute to produce certain chemical modifications of sweat.—See also 'Lancet.'

*A Collection of Parasitic Fungi.*—It seems that under the title of 'Herbarium Mycologicum Œconomicum,' F. Baron Thümen proposes to form a collection of those parasitic fungi which are injurious (including, also, any that are useful) in forestry, agriculture, horticulture, or in any other branch of industry. The specimens of each species will be labelled with the scientific name, diagnosis, and any needful remarks, and, where possible, will be sufficiently numerous for a portion to be submitted to microscopic examination. The collection will be issued in fasciculi of fifty species, at the price of three thalers each, and may be obtained of the collector, at Teplitz, in Bohemia.

*Huizinga's Experiments on Abiogenesis.*—These experiments are particularly interesting, coming as they do at the time that Dr. Bastian has been before the world with his views. Dr. Burdon Sanderson, however, who gave an epitome of them to the British Association, has, we fancy, considerably diminished the force of the arguments in their favour. Huizinga's experiments will be found detailed in full in Pflüger's 'Archiv,' vol. vii., p. 549. Professor Sanderson says that he begins his paper with the words *Multa renascentur quæ jam cecidere*, using them as an expression of the recurring nature of this question. He then proceeds to say that he was induced to undertake his inquiry

by the publication of the well-known work of Dr. Bastian (whom he compliments as having awakened the exhausted interest of physiologists in the subject), his special object being to repeat the much-discussed turnip-cheese experiment.

Everyone knows what Dr. Bastian's observation is. It is simply this, *viz.* that if a glass flask is charged with a slightly alkaline infusion of turnip of sp. g. 1015, to which a trace of cheese has been added, and is then subjected to ebullition for ten minutes and closed hermetically while boiling, and finally kept at fermentation temperature, Bacteria develop in it in the course of a few days. This experiment has been repeated by Huizinga with great care, and the accuracy of Dr. Bastian's statement of fact confirmed by him in every particular: yet, notwithstanding this, he thinks the evidence afforded by these results in support of the doctrine so inadequate, that he, desiring such evidence, has thought it necessary to repeat the experiment under what he regards as conditions of greater exactitude.

Huizinga's objections to Bastian's experiment are two. First, that when a flask is boiled and closed hermetically in ebullition, its contents are almost entirely deprived of air, and (2) that cheese is a substance of mixed and uncertain composition. To obviate the first of these objections, he closes his flasks, after ten minutes' boiling, not by hermetically sealing them, but by placing over the mouth of each, while in ebullition, a porous porcelain plate which has just been removed from the flame of a Bunsen's lamp. The hot porcelain plate is made to adhere to the edge or lip of the flask by a layer of asphalte with which the edge has been previously covered. The purpose of this arrangement is to allow air to enter the flask, at the same time that all germinal matter is excluded. It is not necessary to discuss whether this is so or not.

To obviate the second objection he alters the composition of the liquid used: he substitutes for cheese, peptone, and for turnip infusion, a solution containing in a litre of distilled water—

| | |
|---|---|
| Grape sugar .. .. .. .. | 25 grammes |
| Potassium nitrate .. .. .. | 2 „ |
| Magnesium sulphate .. .. | 2 „ |
| Calcium phosphate .. .. .. | 0·4 „ |

The phosphate is prepared by precipitating a solution of calcium chloride with ordinary sodium phosphate, taking care that the chloride is in excess. The precipitate of neutral phosphate so obtained is washed and then added to the saline solution in the proportion given. On boiling it is converted into soluble acid phosphate, and insoluble basic salt, of which the latter is removed by infiltration. Consequently the proportion of phosphate in solution is less than that above indicated.

To the filtrate, peptone is added in the proportion of 0·4 per cent.

The peptone is obtained by digesting egg-albumen at the temperature of the body in artificial gastric juice made by adding the proper

quantity of glycerin extract of pepsin in water acidulated with hydrochloric acid. The liquid so obtained is first rendered alkaline by the addition of liquor sodæ, then slightly acidulated with acetic acid and boiled. The syntonin thus precipitated is separated by infiltration from the clear liquid, which is then evaporated to a syrup and poured in a thin stream into strong alcohol, with constant agitation. The precipitated peptone is separated after some hours and washed with alcohol, and redissolved in a small quantity of water. The solution is again precipitated by pouring it into alcohol in the same way as before, and the precipitate washed and dried.

Flasks having been half filled with the liquid thus prepared (in 1000, 2 each of nitre and Epsom salts, a trace of phosphate of lime, 25 parts of grape sugar, and 4 parts of peptone), each is boiled for ten minutes, closed, while boiling, with the earthenware plate as above described, and placed as soon as it is cool in the warm chamber at 30° C. The experiment so made "gave, without any exception, a positive result in every case. After two or three days the fluid was crowded with actively moving Bacterium termo."

The readers of 'Nature' are aware that in June last Dr. Sanderson published a repetition of Dr. Bastian's experiments with a variation not of the liquid but of the mode of heating.* Instead of boiling the flasks for ten minutes over the open flame and closing them in ebullition, he boiled them, closed them hermetically, and then placed them in a digester in which they were subjected to ebullition under a pressure of two inches or more of mercury. The result was negative. There was no development of bacteria.

Since the publication of his experiments Huizinga's have appeared. His results, regarded as a proof of spontaneous generation, is clearly not superior to Bastian's. The substitution of a soluble immediate principle for an insoluble mixed product like cheese, and the use of a definite solution of sugar and salts, are not material improvements. The question is not whether the germinal matter of bacteria is present, but whether it is destroyed by the process of heating. Consequently what is necessary is not to alter the liquid but to make the conditions of the experiment as regards temperature as exact as possible. In this respect Huizinga's experiment is a confirmation of Bastian's, and nothing more.

He has recently repeated it with the same modifications as regards temperature as those employed in his repetition of the turnip-cheese experiments. The results have been the same. In all other respects he has followed the method described by him in his paper.

He has prepared the solution of salts, grape sugar, and peptone in exact accordance with his directions. To obviate his objection as to the absence of air, he has introduced the liquid, not into flasks, but into strong glass tubes closed hermetically at each end and only half-filled with liquid, the remainder of the tube containing air at the ordinary tension. Each of these tubes, after having been subjected to the temperature of ebullition under two inches of mercury for half an hour, has been kept since September 10 at the temperature of fermen-

* See 'Nature,' vol. viii., p. 141.

tation (32° C.). Up to the present time, no change whatever has taken place in the liquid.

As a control experiment he opened one of the tubes immediately after boiling, and introduced a drop of distilled water. It became opalescent in twenty-four hours.

In conclusion, he observes that he still maintains his resolution to take no side whatever in this controversy. He does not hold that spontaneous generation is impossible. He does not regard heterogenists as scientific heretics. All he says is, that up to the present moment he is not aware of any proof that they are right.

*Structure of Lichens and Algæ.*—A very capital work has appeared on this subject in Germany. It is a German translation from the Danish of Oersted, and is illustrated by over ninety woodcuts. It is a capital text-book of the Lower Cryptogamia, adapted to the use of ordinary classes or individual students desirous to find their way to a good general knowledge of the structure and classification of *Fungi*, *Lichens*, and *Algæ*. The woodcuts are very striking, and tell their story with great clearness. It is much to be wished that we had something of the sort in this country, in which there is an increasing desire to study the Lower Cryptogamia, but hardly any appliances for it.

*A very large Cuttle Fish* is hardly a subject for a Microscopical Journal; but it is such a curious animal of a very large size, that it may not be uninteresting to note the locality where it is described. The description of this huge beast appears in the first volume of proceedings issued by a scientific society (German) founded in Japan. It states that a specimen has recently been captured there of a large cephalopod of the genus *Ommastrephes* found in the adjacent seas. The length of the Ommastrephes from the point at the hinder extremity to the front edge of the mantle was 186 centimètres (6 feet 1 inch), and 41 centimètres (1 foot 5 inches) more to the mouth. The longer of the eight arms measured 197 centimètres, or nearly $6\frac{1}{2}$ feet.

*The Migrations of White Corpuscles.*—At the recent meeting of the German Scientific and Medical Associations at Wiesbaden some valuable papers were read, a few of which we are enabled to supply from the columns of our contemporary, the 'Academy.' Of these, the first was on the above subject. Herr Thoma described the migration of white corpuscles into the lymphatics of the tongue of the frog. He injected the lymphatics of the living animal with an extremely dilute solution ($\frac{1}{2000}$th or $\frac{1}{8000}$th) of silver nitrate, and found that with certain precautions this did not lead to stasis of the blood in the blood-vessels, but only to a lively exodus of the white corpuscles from their interior. After a time the re-entrance of the corpuscles into the vessels through certain stomata in their walls, marked by a precipitation of the silver, is observed. In a second series of experiments the lymphatics were injected with a dilute emulsion of cinnabar in a $\frac{3}{4}$ per cent. solution of common salt. The cinnabar is in part deposited in the stomata of the lymphatic vessels, partly passes

through them, and is deposited in the tissues in the form of small round cloudy patches. The evidence of the identity of the stomata brought to view by means of cinnabar with those rendered apparent by means of silver nitrate is obtained by their peculiar grouping in the lymphatics of the frog's tongue; and, secondly, by the subsequent injection of silver nitrate into the same vessels. The injection of cinnabar causes very little disturbance of the circulation. If a lively exodus of the white corpuscles from the blood-vessels be produced by making an abrasion of the surface, the migrating cells quickly make their appearance in the stomata of the lymphatics marked out by the cinnabar. They then take up the particles of cinnabar into their interior, which causes them to lose their activity and accumulate in the stomata. They immediately appear in the form of cauliflower-like excrescences projected *on the inside* of the lymphatics, which break up into thin constituents—cinnabar-holding cells. These are seen in motion in the lymphatics, and may be traced thence into the cervical lymphatics and into the blood. In these researches a remarkable uniformity in the track pursued by the white corpuscles was observed. They then pass from the vessels into the tissue by a series of sharp zigzag movements, and all travel at about the same rate.

*The Ramification of Sphacelariæ* was also explained at the Wiesbaden meeting by Herr Pringsheim. According to Herr Magnus, who made some remarks on this communication, there were two modes in which it takes place. In the one the new shoot is formed by an oblique partition cutting off a segment from the youngest cell of the part about to branch. The segment eventually pushed the weaker cell from which it was derived to one side, and a sympodial ramification resulted (*Stypocaulon*). In the other mode a lateral shoot is formed by a bulging out from a cortical cell (*Sphacelaria*), which bulging out subsequently developed into a branch.

*The Structure of the Lamprey's Eye* has been investigated lately by Herr Langerhaus. The globe of the eye in this animal is peculiar in being destitute of any sclerotic coat, and the choroid is directly continuous with the membrana descemetii. In ammocaetes the latter membrane is very strongly developed, and completely fills the anterior chamber. The iris is simply a continuation of the retina, which is attached to the choroid by a thin layer of connective tissue. As Max Schultze has shown, several layers are present in the retina. Inside the external granule layer is found the ganglionic layer, in which a double row of large ganglion cells are separated by a layer of fibres. Within the ganglion layer lie the internal granule layer, the optic-fibre layer, the granulosa, and limitans interna. Processes are given off from the external ganglion layer which penetrate the lamina granulosa externa. The granules of the rods and cones dilate to form cup-like bodies, which likewise stand in connection with the granulosa externa; and these cups are situated, like Hauben (?), upon the processes of the ganglion cells. Thus it is rendered highly probable that there is a direct connection between the connective-tissue cups and membranes of the granules and the connective tissue of the granulosa

externa, and, on the other hand, between the nervous contents with the processes of the ganglion cells.—*Vide Report of the Forty-sixth Meeting of the German Scientific Association.*

*Growth of the Fruit-pedicel of Pellia epiphylla.*—Herr Askenasy described at the above-mentioned meeting the growth of the fruit-pedicel (*seta*) of *Pellia epiphylla*. It was divisible into two periods, during the first of which there was continuous multiplication of cells by division, but scarcely any elongation. On the other hand, during the second period, which lasts only from three to four days, cell multiplication stopped, but the length increased from 1–2 to 80 millimètres. This was accompanied by a total consumption of the starch contained in the cells.

*A Mode of Microscopically Examining the Growth of Plants* is given as follows in a recent number of 'Silliman's American Journal': —It says that the author of the invention, after alluding to Müller's use of a transparent net and Sach's auxonometer, describes a simple device for reaching the same end. He employs a glass tube, of convenient size, to be placed in the field of a microscope, and allows the root or other part of the plant to grow in this. Of course the part must be fixed at some point, either with cork or with damp bibulous paper. The free end of the root has now room for growth, either in water or in moist air—preferably the latter. The tube can be subjected to a known degree of heat by the use of Sach's hot-air chamber (described on page 644 of the Lehrbuch). The tube having been fixed on the stage can be accurately observed every few minutes, or after a longer time, a micrometer being all that is needed for determining distances. The errors which may result from these observations are frankly alluded to. This simple method is particularly adapted to the investigation of the effect of light on growth, as the whole apparatus is completely under control of the observer.

*Occurrence of Starch in Sieve-cells.*—Dr. Briosi communicated to a recent number of the 'Botanical Zeitung' a paper on this subject. A brief recapitulation of previous researches by Hartig and Hanstein is followed by an account of recent original observations. In all plants examined, when a violet colour is produced in sieve-cells by iodine in iodide of potassium, the requisite magnifying power shows that there are always minute granules which present a sharply-defined spherical outline. Even in the so-called solutions of starch in cells, these minute granules can be detected. They remain unchanged after treatment with alcohol and ether. In sections treated according to the method of Böhm-Sachs (that is, heating with a solution of potash, washing, and neutralization with acetic acid, and then addition of a dilute tincture of iodine) the starch granules of the sieve-cells are coloured deeply violet, even when the large starch granules of adjacent cells have become broken down into a paste. If the sections are placed in dilute acids (hydrochloric or nitric) and then treated with iodine in iodide of potassium, the starch granules are coloured blue or deep violet. The minute granules swell up, but still preserve their spherical form, even when the other granules have become a paste.

*Influence of Temperature on Development of Fungi.*—An important paper on this subject has been read before the Vienna Academy during last year by Professor Wiesner. The fungus selected was *Penicillium glaucum*. The germination of the spores (conidia) occurred between 1·5 and 43° C., the development of the mycelium between 2·5 and 40° C., the formation of spores between 3° and 40° C. Near the upper and lower limits, the germination, the growth of mycelium, and the production of spores were uncertain. The rapidity in the rate of germination increases steadily up to 22°, and above that diminishes, at first steadily, then without regularity. The rapidity of mycelial growth rises continuously from the lower limit up to 26° C., and then falls with more or less regularity. The maximum rapidity of the production of the spores is reached at 22° C.

*Microscopic Investigations on Pyæmia.*—Those which have been published in one of the numbers of the 'British Medical Journal' have been made by Dr. Birch-Hirschfeld, and were presented to that journal by Dr. Dreschfield of Manchester. Dr. Birch-Hirschfeld, on examining daily the pus coming from a wound, found that, with the ushering in of the first symptoms of pyæmia, the pus also showed a corresponding change, consisting in the presence of micrococci, either in pairs, strings, or colonies (the latter especially when pyæmia was far advanced or rapid in its course), and in an altered appearance of the pus-corpuscles, which were finely granular, of less definite outline and lustre, and which showed their nuclei very distinctly without the addition of any reagent. The *blood* of such pyæmic patients contained similar micrococci, and its white corpuscles had undergone a change very similar to that of the pus-corpuscles. Sometimes the pus of a pæmic patient would contain, besides these, a quantity of the *Bacterium termo* or *Bacterium lineola*, which are the common bacteria of most putrescent matter; while micrococcus is, according to Cohn, Klebs, and Hirschfeld, not to be considered the ferment of putrefaction. Healthy pus coming from a healthy wound or from a simple abscess showed no micrococci and no altered pus-corpuscles, while putrescent pus (either after exposure to air or coming from an unhealthy or gangrenous wound) contained only the bacteria (termo, lineola, and bacillus) due to putrefaction. The difference between pyæmic and putrescent pus was now further shown by inoculations on rabbits. Healthy pus, injected subcutaneously into a rabbit, gave rise only to a local abscess, without any further disturbances. Putrescent pus gave the symptoms of septicæmia, as described by Bergmann, Sanderson, and others—larger quantities only being fatal, and the fever appearing almost immediately after injection, showing the sepsis curve of Bergmann very well; while pus from a pyæmic patient, similarly introduced into a rabbit, gave rise to a different course of symptoms. The animal remained well for five or six days; and this period was followed by one of high and intermittent fever, diarrhœa, emaciation, and eventually and almost invariably by death from the sixteenth to the twenty-fourth day. Pus, blood, and the metastatic changes in such rabbits, showed again all the distinctive pyæmic properties described.

*Is there any Connection between the Cells of Glands and the Nerves distributed to them?*—According to some recently-conducted researches on this point by Dr. C. Kupffer, Pflüger's well-known view of the termination of the nerves in the cells of the salivary glands receives considerable support. Dr. Klein gives a report in the 'Medical Record,' in which the following account is given of Herr Kupffer's paper in Max Schultze's 'Archiv.' * He states that Dr. Kupffer, in support of the assertion of Pflüger, that the nerve-fibres of the salivary gland terminate in the cells of the acini, describes the distribution and termination of nerve-fibres in the salivary glands of the larvæ of muscidæ, and in those of blatta orientalis. In the former the salivary glands represent two large almost isolated simple cylindrical tubes, each of them consisting of a membrana propria and of hexagonal transparent finely granular nucleated cells, which cover that membrane and line the central canal of the gland. The nerves that provide these glandular tubes come to the glands neither as isolated structures nor as accompanying the duct, but together with the corresponding trachea. These, having reached the gland, surround it by numerous branches, which perforate the propria and ramify between the hexagonal cells. Under a high power (Hartnack, No. 10) they are seen to give off fine pale fibrils, provided with numerous nodular swellings, which penetrate into the cells themselves. Kupffer takes these minute fibrils to be nerve-fibrils. There are, however, also ultimate branches of the tracheæ, which, being filled with air, are easily recognizable as such, and having penetrated the cells, may be followed up to their nucleus. More satisfactory were the observations on the large salivary glands closely attached to the œsophagus of blatta orientalis. Those glands are richly provided with nerves, which form plexuses in the interstices of the lobules. From these plexuses numerous nerve-fibres run to the acini of the gland; at the point of joining these (acini) they form a conical dilatation, in such a way that the connective-tissue sheath of the nerve-fibre becomes continuous with, and the distinctly fibrillar axis-cylinder perforates through the membrana propria of the acini. After the perforation, the individual fibrils of the axis-cylinder penetrate the cells, and there they terminate. Under high powers (800), it can be seen that most of the fibrils run to the cells lying more inwards. The fibrils do not coalesce with the substance of the cells, but are quite distinct from this latter for some distance, while pursuing their course in it. On their way they divide sometimes dichotomously. These nerve-fibrils do not terminate in the nucleus, but run towards a vesicular structure, which is imbedded in each of the peripheral cells, and which represents the intracellular termination of the duct. The mode of termination is not finally made out. To demonstrate these relations, it is best to place the fresh œsophagus with the adherent glands on a glass slide, and to expose them to the vapours of perosmic acid in substance for a few minutes, until the object has assumed a brownish colour. The lobules of the glands may then be isolated without diffi-

* Vol. ix., part 2.

culty. Also the examination of the fresh gland in iodine serum is of great advantage.

*Dr. Pigott on the Podura Scale.*—Dr. Pigott, F.R.S., in conjunction with Mr. E. B. Beaumont, F.R.S., has published his observations on the structure of the Podura scale, in the 'Proceedings of the Royal Society' (in a recent number). He says, speaking for both authors, that nothing in microscopic matters has ever afforded us such complete satisfaction as the following result of a very fine definition, accomplished by means of a Gundlach German $\frac{1}{16}$th immersion lens, corrected by a new method, which Dr. Pigott at present delays publishing in the hope of further improvement, but which he is willing to exhibit at his house. The idea conveyed by looking at the object which is figured in the 'Proceedings' was, that two layers of spherules (first detected by Mr. Beaumont within the tubes), like two confined layers of small shot, had, by compression, been forced and largely spread out into broader layers. It was thought also that detached portions resembled long tubes or puckers filled with spherules exactly fitting them. The spherules appeared perfectly spherical, but somewhat unequal in size. In the general flattened and extended surface of the compressed and disintegrated scale the spherules appeared dark blue or red, according to the slight change in the focal plane, and in a still lower plane white. In the adjoining uninjured scales long strings of beads were seen, like necklaces of coral, here and there sharply bordered with black lines, apparently denoting tubes of membrane or puckers enclosing them like a tube. Between these strings of spherules peeped forth others of a light orange-colour. The slide was an old one and well known. The mass of the crushed scale occupied a much broader space than any of the scales.

*Mr. Sorby's recent Researches in Vegetable Chromatology* are of great interest. They are published at very considerable length in the 'Proceedings of the Royal Society,' and though not many of them come within the rank of microscopy, yet they are of great chemical and physical value. With regard to Fucoxanthine the following remarks are made:—"This is the name I propose for the principal colouring-matter of *Fuci* and other olive *Algæ*. It may be obtained in the manner already described, only that in order to separate it from the chlorofucine, after separation of all the chlorophyll, a few drops of ammonia should be added to the alcoholic solution, and the whole diluted with an equal bulk of water. The bisulphide of carbon is then precipitated with almost all the fucoxanthine, whilst nearly the whole of the chlorofucine remains in the dilute alcohol. As thus purified, fucoxanthine dissolved in bisulphide of carbon is of a beautiful amber colour, and its spectrum shows two obscure absorption-bands, the position being intermediate between those of orange xanthophyll and xanthophyll, so that a mixture of these gives nearly the same spectrum. The difference, however, is completely proved by other facts. The bands of fucoxanthine are much less raised by alcohol and other liquids of high band-raising power than the bands of those two kinds of xanthophyll, and it resists the action of light far

more than they do. A mixture of pure orange xanthophyll and xanthophyll in absolute alcohol treated with hydrochloric acid would, at the most, give only a pale green, whereas fucoxanthine is changed into a splendid blue substance, and subsequently into a sort of claret-coloured, before finally and slowly fading. Though the spectra of fucoxanthine and yellow xanthophyll are essentially different, yet this blue product is the same. It absorbs the whole of the red end of the spectrum, not transmitting even the extreme red; and on adding an excess of ammonia this absorption is entirely removed, and the colour is changed to a bright yellow. The spectrum then shows a well-marked absorption-band at the violet end of the blue. On adding excess of hydrochloric acid, the original blue colour is restored: and hence this substance has the unusual peculiarity of being made blue by acids and yellow by alkalies. Hitherto I have never met with it in plants themselves. Taking everything into consideration, we must look upon fucoxanthine as closely related to xanthophyll; but at the same time the different effect of solvents in raising the absorption-bands, and the greater permanence when exposed to light, may perhaps make it desirable to class it in a subgroup. The dull olive colour of those *Algæ* in which it occurs so abundantly (the *Melanospermæ*) is apparently mainly due to it in a *free state*, not dissolved in any oil. On comparing the spectrum of the light transmitted by a frond in its natural condition with that of the light transmitted by a portion which has been boiled for a short time in water until the colour has changed to green, it may be seen that the absorption due to the fucoxanthine is considerably raised, just as if at that high temperature it were attacked and dissolved by the oil present in the plant."

*The Production of the Macrogonidia in the Genus Hidrodictyon.*—Dr. Horatio Wood has recently published a work on the fresh-water Algæ of America (a book we trust soon to have for review in these columns), from which the editor of 'Grevillea' gives some lengthy quotations in a recent number of his journal.* The following relates especially to the above subject, and is but a small part of the original excerpt:—"The investigation of the production and development of the *macrogonidia*, however, has occupied considerable of the time devoted by myself to the microscope, and I have seen large numbers of specimens in almost all the stages of development. I have never been able to detect, however, any decided motion in the *macrogonidia*. They are formed in the protoplasmic stratum already alluded to as occupying the outer portion of the interior of the *Hydrodictyon* cell. The first alteration in this, presaging their formation, is a disappearance of the starch granules, and a loss of the beautiful, transparent green colour. Shortly after this, even before all traces of the starch-grain are gone, there appear in the protoplasm numerous bright spots placed at regular intervals; these are the centres of development, around which the new bodies are to form. As the process goes on, the chlorophyll granules draw more and more closely around these points, and at the same time the mass becomes more and more opaque, dull, and yellowish brown in colour.

* Oct. and Nov., 1873.

This condensation continues until at last the little masses are resolved into dark hexagonal or polygonal plates, distinctly separated by light, sharply-defined lines. In some the original bright central spot is still perceptible, but in others it is entirely obscured by the dark chlorophyll. The separation of these now becomes more and more positive, and they begin to become convex, then lenticular, and are at last converted into free, oval, or globular bodies. When these are fully formed they are said to exhibit a peculiar trembling motion, mutually crowding and pushing one another, compared by Mr. Braun to the restless, uneasy movement seen in a dense crowd of people in which no one is able to leave his place. Whilst the process just described has been going on, the outer cellulose wall of the *Hydrodictyon* cell has been undergoing changes, becoming thicker and softer and more and more capable of solution, and by the time the gonidia are formed it is enlarged and cracked, so that room is afforded them to separate a little distance from one another within the parent cell. Now the movements are said to become more active—a trembling jerking, which has been compared to the ebullition of boiling water. There is, however, with this a very slight change of space, and in a very short time the gonidia arrange themselves so as to form a little net within the parent cell, a miniature in all important particulars of the adult *Hydrodictyon*. The primary cell wall now becomes more and more gelatinous, and soon undergoes complete solution, so that the new frond is set free in its native element. As previously stated in my investigations, I have never seen the peculiar motion above described, the newly-formed gonidia simply separating and arranging themselves without my being able to perceive any motion, or exactly how they fell into position."

## NOTES AND MEMORANDA.

**Mr. Browning's Stage-and-body-united Microscope.** — Our readers will remember that we gave a short notice with a cut of this new instrument which Mr. Browning has manufactured under the direction of Mr. Mayall, jun. When describing it we spoke of "the disadvantage of a monocular as compared with a binocular instrument." This disadvantage has since been completely overcome. Mr. Browning has made several of this form of microscope of the binocular kind, and finds that they give perfect satisfaction. There is therefore reason to rejoice more than ever at the introduction of the instrument.

**Scientific Societies' Club.**—Although this is not a microscopical subject, it is one which as fairly interests Fellows of the Royal Microscopical as any other Society. We therefore do not hesitate to introduce it to our readers' notice. For some time past Mr. J. Logan Lobley has been exerting himself in the post of Hon. Secretary to this Institution, and he has been, we must say, wonderfully successful. At the meeting which was held by the provisional committee in

November last, it was decided among other things that original members, who shall have been members of the provisional committee, or founders, to the number of 250—entrance fee, two guineas. Annual subscription: town members, two guineas; country members, one guinea. Members entering after the club is founded—entrance fee, four guineas. Annual subscription: town members, three guineas; country members, one guinea and a half. Although these fees are small when compared with those of other clubs, a large number of members will furnish an ample income for an economically conducted and moderate establishment. When the requisite number of names has been received, a general meeting will be called to formally found the club, but until then it is not proposed to incur any beyond very trifling preliminary expenses. Gentlemen sending in their names to be added to the provisional committee will consequently be under no pecuniary or other liability. It is therefore hoped that the proposal may meet with the general support of scientific men, and that thus a scientific societies' club may be successfully established. There have been already more we learn than a hundred members, so that we trust soon to hear that the lists have been quite filled up. Gentlemen who are desirous of joining will please forward their names to us, when we shall be happy to communicate to them any facts or information regarding the club which they may require.

## CORRESPONDENCE.

### Cements.

*To the Editor of the 'Monthly Microscopical Journal.'*

Dear Sir,—In the report of the microscopical meeting of the Brighton Society the subject of discussion was "Cements." As I have made many experiments for the purpose of discovering a permanently adhesive material, I may perhaps be allowed to make a few remarks on the subject. For dry mounting, where only very shallow cells are required, I have found nothing better than asphalte dissolved in benzole, with a small quantity of gold size added. The cells should be made by the addition of successive layers of varnish, each layer to be hardened before the next is put on. When thick enough, the slides should be placed in a cool oven and allowed to remain all night. In order to attach the cover, I put a fresh layer of asphalte (without gold size) on the surface of cell, and allow it to remain exposed five or six minutes. The cover may now be placed upon it, and pressed upon the cell by a slide, previously heated to ensure perfect contact, and it may now be finished off with an exterior ring of the asphalte (No. 2); or if it is wished to put a coloured ring or rings round it, a layer of ordinary shell-lac varnish should be run round it before using them. I have found the dammar cement, made by Mr. W. White, of Litcham, the best medium for mixing the colours with;

a few drops of gold size may be added with advantage. I would advise the use of zinc white, in preference to white-lead, as the latter turns yellow in the course of a month or two. I give the preference to vermilion or purple lake for the exterior ring, finishing with an interior ring of zinc white.

My experience does not confirm that of Dr. Hallifax; I always find sealing-wax varnish to become brittle in the course of a year or two.

The following cement is given in 'The Microscope' by Dr. Frey. *Thiersch's cement.*—Dissolve shell lac in spirit of wine, in sufficient quantity to make a thick varnish, colour with a concentrated solution of aniline blue or gamboge, in absolute alcohol; add about a scruple of castor-oil to each ounce of mixture. After some further evaporation it must be preserved in a well-closed vessel. Previous to using this cement the inventor directs that the edges of balsam-mounted slides should have a layer of balsam dissolved in chloroform, put round them in the same manner as asphalte, and at least three days, but still better, weeks or months should be allowed to elapse before applying the cement.

Yours very truly,

F. KITTON.

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## POTATO BLIGHT.

*To the Editor of the 'Monthly Microscopical Journal.'*

LONDON, *Dec.* 12, 1873.

SIR,—It is remarkable how little has been done with the microscope for the investigation of the origin and spread of the potato blight. This, like many others of a similar character, has been rather hastily attributed to the growth of a fungus—I consider without sufficient reason.

A fungus, from the universal presence of the spores in damp localities, and its rapid growth, may appear simultaneously with morbid conditions, and yet not be the primary cause. The vine disease, being cuticular, may be readily traced by the microscope to a fungoid origin, further proved by the well-known sulphur cure, so destructive to fungi in confined localities. This is of no avail in the potato disease, which, under conditions favourable to its development, is internal and constitutional.

On placing a very thin slice of potato (taken at any time of the year) under the microscope, the cells are seen filled with starch granules, and the walls coated with a layer of active protoplasm of the usual molecular appearance. In the healthy cell, this protoplasm (the vital principle in all plants) when seen under the highest powers, with suitable illumination, has a vibratory motion, with feeble currents, in various directions. On approaching the vicinity of the diseased portion, the cell walls begin to appear of a light brown colour, and wherever the least tinge of this becomes apparent, there is no movement, nor can any protoplasm be detected adhering to the wall of the cell,

which from that time is a dead member. Tracing the cell walls further the colour deepens, and the septa become thicker, till at last the walls split, giving the now rotten cell a detached appearance; but from the first indication of disease to the final rotten state, no vital activity can be discovered. In all the phases the starch granules remain unaltered, completely resisting this peculiar decomposition. The disease is evidently conducted throughout the tuber, in the substance of the cell walls. Of its origin I offer no opinion. If it arises from a deficiency of the vital principle or protoplasm, or a want of stamina (so to term it), the microscope might discover this by a long series of comparisons, for the presence of protoplasm is necessary for the preservation and growth of the cell wall.

Your obedient servant,

F. H. WENHAM.

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## THE MERITS OF THE APLANATIC SEARCHER.

*To the Editor of the 'Monthly Microscopical Journal.'*

THE HAYES, STROUD, GLOUCESTERSHIRE, *Dec.* 12, 1873.

SIR,—I must apologize for so soon trespassing again on your valuable space; and should others have written to you on the subject of my letter, there will be no occasion whatever for its appearance in your Journal. Now, some of your readers (and perhaps Mr. Mayall amongst them, when he wrote to you in the December number) may not have forgotten an able paper in the January number of 1871, read before the Microscopical Society by Mr. McIntire, "On the Minute Structure of the Scales of certain Insects," where, after the most careful investigation of its merits, and earnest wish to do justice to them, he finally decides *against* the practical utility of the "Aplanatic Searcher." As the article in question can easily be referred to, it is needless to make any quotations from it here; but it is somewhat irritating to find a piece of apparatus still held up for admiration, after it has been determined nearly three years ago, and probably up to this time, by hundreds of microscopists, that it lacks just that one special advantage as a corrector of the defects of an object-glass so triumphantly claimed for it. A substitute for a high power can indeed be conjured up, by placing a 1½-inch objective between the eye-piece and a 1½th, but this can only be procured at the sacrifice of what is so greatly prized in an object-glass—sharpness of definition. There is as much difference between the results obtained from the one and the other, as there is between the finest line of the graver on copper plate and the blurred mark of a chalk pencil on a piece of paper.

Improvements, moreover, in the optical part of the microscope, as is often the case in other optical and mechanical arrangements, are gained rather by simplifying structure already complex and intricate, than by a contrary proceeding. Granting the additional media required, beyond those of a single lens, doublet or triplet, for securing the necessary conditions of a good object-glass, then, surely, the more

simple the construction the better. The comparatively modern 1-inch, the new 5-lens object-glass, the Coddington magnifier, and the old doublet and triplet of Wollaston and Holland, are all instances of this, and others might be cited. I shall, however, content myself with quoting the written opinion of a great optician, now no longer amongst us, who, when asked what he thought of the "Aplanatic Searcher," then just brought into notice, replied, "I consider the arrangement quite a mistake, and the views on the subject to be *manifestly* erroneous." Not even a trial of it was required. The microscope of to-day gives better results through fourteen surfaces of glass, than when, two years ago, it consisted of twenty—why therefore add eight more to them?

On the whole, then, I think Mr. Mayall and most of your readers will agree with me that it would be as well if some microscopists would be more careful in testing accurately the practical excellence of their discoveries, before attributing to them an importance far beyond their due, and give credit to others for rejoicing in the possession of at least as perfect object-glasses as they themselves possess.

Believe me, faithfully yours,

J. J. Plumer.

## PROCEEDINGS OF SOCIETIES.

### Royal Microscopical Society.

King's College, *December* 3, 1873.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read, and the thanks of the meeting were voted to the donors.

The President said that the Society had already been informed of the intention of Mr. Charles Woodward to bequeath to them a valuable and complete Smith and Beck's microscope; but, instead of making it a bequest, Mr. Woodward had presented it to the Society. He therefore proposed that the special thanks of the Society be presented to Mr. Woodward for his valuable donation. Motion put to the meeting and carried unanimously.

The President announced that the Society would meet for a scientific evening on December 10th, and expressed a hope that Fellows would make it as interesting as they could by bringing objects of interest for exhibition on that occasion.

The Secretary read a paper, by the Rev. W. H. Dallinger, "On some Further Researches into the Life History of the Monads," being in continuation of the paper upon the same subject read at the November meeting of the Society. The paper was illustrated by a series of beautifully-executed drawings, and it was intimated that the effects of

temperature in the forms examined would be described in a future communication. The paper will be founded printed at p. 7.

The President, in proposing a vote of thanks to Mr. Dallinger and Dr. Drysdale, expressed his opinion that their paper formed an important contribution to the Society's proceedings. The whole process of development seemed to have been very clearly made out, and it had been shown that, in addition to multiplication by fission, sometimes three or four monads became connected, first forming one body, afterwards dividing and subdividing until the interior became filled with multiple bodies, that the sac then burst, and a number of oval bodies were set free, which in turn developed and produced a new progeny.

The President, in reply to a question from Mr. Sopwith, stated that the drawings which accompanied the paper would be engraved and printed in the next number of the Journal.

Mr. Charles Stewart said that it might perhaps seem rather fanciful to draw a parallel between the development of so low an organism as these monads and ourselves. Yet he thought he could trace a degree of similarity in the earliest stages of development. It appeared that two masses of protoplasm became fused, and then there occurred a power of amœboid motion, followed by a division of the common mass within the envelope. Now, in the development of ourselves almost the same thing occurs; two masses of protoplasm unite and form a fertilized egg. As growth proceeds this becomes divided, first into four, then eight, and so on, until the result is a great number of embryo cells within the same envelope; but, in this case, they were all more or less dependent upon each other, and they did not escape, although, for a very long time, they might exist in a state of separation. There was a well-known experiment which would illustrate this. If some of the white blood corpuscles were taken, and carefully closed up in a cell to prevent all evaporation, they would keep alive for weeks, or even months, occasionally swallowing up a red corpuscle, and behaving exactly the same as an amœboid animal.

A vote of thanks to the Rev. W. H. Dallinger and Dr. Drysdale was then put to the meeting and carried unanimously.

Mr. Charles Stewart called the attention of the meeting to a leaf section exhibited under a microscope in the room, to show the cystoliths existing in some of the cells, and gave the following particulars respecting it. The object was a vertical section of a very common leaf—that of the India-rubber plant (*Ficus elastica*) so frequently grown in pots as an ornament in rooms. The particular direction in which the section was cut was at right angles to the nerves, which radiated on either side from the great central midrib of the leaf, and he recommended a section cut in this direction, because a very much prettier object was obtained in that way than in any other, whilst, at the same time, the special portions which it was desired to show were equally well seen. His method of preparation was quite simple, but might still be interesting to some who were present. To obtain the section, he first cut a notch in a carrot, and having inserted the piece

of leaf, he cut slices of both carrot and leaf together. Having got the sections in the way, he placed them in a watch-glass with some water, and then put them into the exhausted receiver of an air-pump in order to extract every particle of air from them, the presence of which would prevent the staining fluid from thoroughly permeating the object. Having thus obtained sections completely saturated with water, the next thing was to stain them. This was done with hæmatoxylin, which produced the familiar violet tint which was so much less fatiguing to the eye of the observer than carmine. When stained, the most important thing to be done was to keep it permanently for future observation. To do this it was first taken out of the staining fluid and washed in water, and after as much as possible had been allowed to run off, a little absolute alcohol was poured over it. This was in turn got rid of by drops of oil of cloves. But here a precaution must be observed. Directly the oil of cloves was put on, a heavy covering of glass must be placed over, and it must be left under pressure until it cleared, otherwise it would curl up and be entirely spoilt.

Its features as an object were certain curious little concretions, called cystoliths, lodged in some of the cells, and the question was, what were these? If the leaf section were examined, it would be seen that immediately below the surface of the leaf was a row of small cells, known as the epidermic cells. Beneath these were two sets of larger cells, the continuity of which was interrupted occasionally to give place to a very much larger cell, which extended deeper down into the softer tissue of the leaf. This was the cell which contained the little *cystoliths*, which were found suspended by a kind of peduncle from the upper end of the cell (diagram drawn and explained). There were, of course, other kinds of cells in the leaf, but it would not be necessary then to take them into consideration. The question was, what were these little concretions? Were they really composed of crystals of lime, or were they merely of organic nature? The first idea about them seemed to have been expressed by Meyen, in 1827. He considered that the stem was composed of cellulose, and that the cystolith was composed of gum covered over with crystals of carbonate of lime. Another idea was that it was a concretion formed at the base of an abortive hair. He thought, however, that there was some doubt as to its being a concretion at all, composed of lime salts. He was rather inclined to look upon it as being an organic concretion of a gum-like nature, and for these reasons:—1. Because when a thin section was made and was looked at in water, it was seen to be highly refractile; but in about twenty-four hours the whole of this brilliancy disappeared, and it looked just like a mass of gum. 2. Because, if the process of staining were prolonged, the entire substance of it would become intensely stained. And 3. Because it did not show any appreciable power of double refraction when examined in polarized light. He had not tested it with acid, so could not say if any action was produced upon it in that way. He thought that, for these reasons, the *cystoliths* might be regarded as being composed of cellulose, or of some gum-like material deposited upon a cellulose stem. As regarded

the functions of these bodies, very little seemed to be known, although it had been thought that, like raphides, they might be excretions, and that the plant got rid of certain waste products in this way. Like raphides, they did not appear to perform any particular function in the economy of the plant; but it was, of course, quite possible that they might have some function unknown to us, or may have had one in past time, or, perhaps, may have a function to perform at some time to come.

A vote of thanks to Mr. Stewart for his communication was unanimously passed.

Mr. Shadbolt suggested that it might be just possible that the cell had at one time contained a secretion, which had, in course of time, contracted upon the peduncle from the loss of aqueous matter. Supposing the cell to have been, at one time, filled with a gummy secretion, it might become contracted in this manner by evaporation.

Mr. Charles Stewart said, that by looking at a young living leaf, it would be found that this idea would not hold good—it would be quite evident that there was originally a peduncle, and that layers were deposited around it, which, in course of time, lost their original character. It was quite clear that the matter, whatever it might be, was deposited layer by layer upon the peduncle.

The President said it struck him that there might be some relation to the special secretion of the plant, the well-known elastic gum.

Mr. Frank Crisp inquired if these bodies were mentioned by anyone as being common to other plants as well as this one.

Mr. Charles Stewart said there were many other plants which contained them—one interesting form was found in the leaf of *Böhmeria nivea* (drawn on black-board), another occurred in *Pilea densiflora*; and there were others.

In concluding, Mr. Stewart stated that he did not pretend to originality in the above remarks, they being only made to explain the slide exhibited. Those interested in the subject may refer to the 'Annales de Sciences Naturelles, Botanique,' 4th ser., vol. ii., p. 267.

The meeting was then adjourned to January 7th, 1874.

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*Scientific Evening.*

The first scientific evening of the session was held in the great hall of King's College, kindly lent for the purpose, on December 10th, when, notwithstanding the dense fog which enveloped London, between fifty and sixty Fellows and their friends attended, many of whom expressed themselves highly pleased at having the opportunity of seeing the numerous objects of special interest which were exhibited. The Society was again indebted to Mr. Baker and Mr. How for kindly sending a large number of excellent lamps.

The subjoined list is not as complete as could be wished, as several exhibitors omitted to supply names or descriptions of their objects.

The Society exhibited *Batrachospermum moniliforme* and a species of *Draparnaldia* from Philadelphia, mounted and sent by Mrs.

Quimby. A small tank sent by Lord S. G. Osborne on September 15, apparently quite dry, in which state it had been kept until the morning of the meeting, when it was filled with water, and found to contain a large colony of *Philodina roseola*, which almost immediately became very lively.

Mr. W. N. Hartley, of King's College: Potassio-calcium chromic oxalate. A remarkable example of pleochröism. Though of a reddish violet tint by gaslight, it appears by daylight, or the light of magnesium wire, to be a mixture of blue and green crystals. The substance transmits rays of different colours through its different crystalline arcs.

Mr. John E. Emary: A new revolving amplifier. This apparatus consists of a metal disk, containing a series of plano-concave lenses, of different foci, which is made to revolve, so as to bring the centre of each lens within the centre of the body. The lenses have the effect of greatly increasing the magnifying power, at the same time that they give a greater working distance between the object-glass and the object, and do not strain the eye like deep eye-pieces. The lenses can be changed and different powers obtained with the utmost facility.

Mr. W. A. Bevington: Head of *Tænia denticulata;* and a Stephenson binocular microscope.

Mr. John Browning: Precious opal; shell of insect's egg from Uruguay; and his new rotating bar microscope.

Mr. Thos. Curties: Sections of jaw-bone of cat and rabbit, with teeth *in situ* (injected).

Mr. Hailes: Foraminifera.

Messrs. How: *Micrasterias denticulata;* and a Lepralia.

Mr. W. T. Loy: *Bombyx mori* (larva), dissected, and showing the arrangement of the cutaneous muscles, nervous ganglia, dorsal vessel, and trachea.

Mr. S. J. McIntire: Eye of dragon-fly (*Libellula*), opaque, showing the arrangement of the pigment cells, which cause the beautiful blue tint seen in the eye. These cells are immediately below the cornea.

Mr. William Moginie: Travelling binocular microscope, ova of toad, and palate of trochus, &c.

Mr. Walter W. Reeves: The fly-mould, *Saprolegnia ferax*, and the imperfect terrestrial condition of the fungus, kindly supplied by Mr. James Renny.

Mr. E. Richards: Royal star coral (*Balanophyllia regia*), alive, with protecting cap on the objective.

Messrs. Ross: Heliopelta, and scale of Podura, showing broken ribs, with their $\frac{1}{25}$th objective.

Mr. Charles Stewart: Respiratory apparatus of Ascidian young feather-star (*Antedon*); and young *Echinus miliaris*.

Mr. Amos Topping: A very beautiful section through both eyes of a dragon-fly (*Libellula*).

Mr. F. H. Ward: Crystallization at various temperatures of mixed sulphates of copper, iron, zinc, and magnesia.

Mr. T. C. White: Parasite of sole (*Caligus*).

Mr. Edward Wright: An electro-magnetic turn-table.

Donations to Library, &c., to December 3rd, 1873:—

| | From |
|---|---|
| Land and Water. Weekly | *The Editor.* |
| Nature. Weekly | *Ditto.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal | *Society.* |
| Quarterly Journal of the Geological Society, No. 116 | *Ditto.* |
| Bulletin de la Société Botanique de France | *Ditto.* |
| A large Binocular Microscope, with all the Powers and Apparatus, Mahogany Case, &c., &c. | *Chas. Woodward, Esq.* |

WALTER W. REEVES,
*Assist.-Secretary.*

## READING MICROSCOPICAL SOCIETY.*

*Nov.* 4, 1873.

In addition to other objects of interest, Captain Lang exhibited a sporangial form of *Orthoseira Dickeii* and *Liparogyra spiralis* (from moss) sent to him by Mr. Kitton.

Mr. Tatem exhibited male flea of hedgehog, acari of stag-beetle, feet of *Xylacopa* from Ceylon, and *Kolpocephalon*, a parasite of the kingfisher.

*Dec.* 2, 1873.

Mr. Tatem laid before the Society a series of drawings illustrating some phases of the development of the *Heineta* of *Epistylis nutans*. He also showed balsam and glycerine mounted specimens of the acarine parasites of *Obisium* and Gamasus from Mr. McIntire.

## MEDICAL MICROSCOPICAL SOCIETY.

At the ninth ordinary meeting of the Medical Microscopical Society, held at the Royal Westminster Ophthalmic Hospital, on Friday, Nov. 21st, Jabez Hogg, Esq., President, in the chair, the minutes of the previous meeting were read and confirmed.

Dr. Bruce described at some length the various methods of studying inflammation.

Dr. Bruce considered the description of the *modus operandi* of observing inflammation in the frog's foot was useless for two reasons:

1. The epithelial surface soon becomes dim with the action of reagents, so as to obscure the vessels.

2. The vessels are not altogether suitable, and, besides, there is sometimes difficulty in stretching the web between the toes without interfering materially with the circulation; he therefore preferred the mesentery—and recommended Hartnack's microscope—beginning the examination with a low power, and afterwards using Hartnack's No. 7 objective, which is equal to an English $\frac{1}{4}$-inch magnifying power.

The frog plate should consist of a piece of glass with a cork (having a circular hole in the middle and covered with a small cover-glass) cemented with sealing-wax to the one end of it. The mesentery is then pinned out upon the cork over the glass.

The frog should be injected with 1 minim of a $\frac{1}{6}$ per cent. solution of curara subcutaneously, because this paralyzes all the muscles except

* Report supplied by Mr. B. J. Austin.

the heart; then make an incision along the right side of the body, about an inch in length, in a line with the leg and arm—avoiding all blood-vessels, so as to prevent blood corpuscles getting on to the mesentery—then draw out the intestine, and having placed the mesentery on the cork plate, moisten its surface with salt solution. It is best to expose the mesentery for three hours before the observation be made, and a large vessel is best for examination. The chief difficulties which may be experienced are (*a*) imperfect curarization, (*b*) adhesions, or (*c*) tearing the mesentery.

The mesenteries of warm-blooded animals had been used by Stricker on his large warm stage; but Dr. Bruce had no experience with them himself, and their examination was attended with a good deal of difficulty.

The tongue of the frog is useful for studying inflammation. Cohnheim first used it, placing the frog on its back, and observing the dorsum of the tongue, in which he excited inflammation by snipping the mucous membrane; caustic has also been used for this purpose. Cutting the mucous membrane, however, gives rise to hæmorrhage; therefore Cohnheim prefers making use of the under surface, in which he causes inflammation by passing a ligature round the root of the tongue. At the end of forty-eight hours he undoes this, and then white blood corpuscles are seen to be passing freely through the vessels. Dr. Bruce has found, however, that after ligature the circulation does not always recover. To prevent the ligature injuring the tongue, it is best to place a piece of leather between the ligature and the tongue. Dr. Bruce also referred to the tail of the tadpole, the wing of the bat, and the cornea of the rabbit, &c., as structures in which inflammation may be observed, and then concluded by asking an opinion as to the origin of pus; whether the members held with Cohnheim, that all pus comes from the vessels or from the connective-tissue corpuscles, or from both sources.

The President proposed a vote of thanks, and after eulogizing Dr. Bruce's remarks, stated his opinion of the value of investigating living tissues, that it would probably be the only means of advance in pathological research. Paget even gave but crude information on inflammation, while Cohnheim has elucidated much in living tissues.

Dr. Payne referred to the difficulty of Cohnheim's experiment on the mesentery. He also considered that Virchow's idea of the origin of pus, though now old-fashioned, was far from being overturned by Cohnheim, and that in inflammation of mucous surfaces we see instances of small cells in larger (mother) ones, though he acknowledged that it might be true that the small cells migrated into the larger ones. In the cornea proliferation has been seen. One view may be taken of all these structures, *viz.* that some parts of the body show greater tendency to reproduction than others, and especially those of embryonic character. He had heard Virchow state that the more perfect endothelium of the peritoneum could not go on producing other elements, while the more simple endothelium of the lymphatics might do so.

Dr. Payne then stated that at present only a small class of tissues had been studied in a living and inflamed condition, and until all tissues have been examined, it was not fair to speak generally on the

subject. The escape of colourless blood corpuscles is undoubtedly an abundant source of pus, whatever other origin it might have.

Dr. Evans asked what effect curara had on the tone of the vessels.

Mr. White asked if Dr. Bruce had tried the effect of chloroform vapour on a curarized frog, because he had noticed a regurgitation or stoppage in the circulation as a result.

Mr. Schäfer preferred the mesentery of the toad to that of the frog, because it is longer, and has a lymphatic sac in its centre.

Mr. Churton considered that these experiments were not calculated to forward our knowledge of the treatment of inflammation in man, but rather to show the origin of pus.

Dr. Matthews suggested the use of a spring-clip instead of a ligature for the frog's tongue, and said that the use of curara might be obviated by immersing the frog for a short time in warm water.

Dr. Bruce in reply stated that he was not aware that curara had any influence upon the process of inflammation. He had not tried chloroform for frogs. He also stated that the same result as Dr. Matthews produced by placing a frog in warm water might more conveniently be brought about by holding the frog in the hand for a few minutes.

Mr. Needham then showed his modification of Dr. Rutherford's microtome, which consisted in its having a movable glass plate on the upper surface, through which the cylinder containing the imbedded specimen projected nearly, but not quite, to the level of the cutting surface.

Dr. Matthews showed another modification of the same microtome, and also a diagonal razor, with the shoulder ground down flush with the rest of the blade, which he found more handy than the ordinary razor.

A vote of thanks was accorded to both these gentlemen.

Mr. Miller advocated the use of a steel plate for the upper surface of a microtome, the great drawback to its use being the liability to rust. He preferred a thick razor.

Mr. Clippingdale showed a micro-spectroscope, in which two spectra could be compared in one and the same field.

Mr. Kesteven described a method of microscopic drawing in which the neutral-tint glass of Dr. Lionel Beale's reflector was removed, and an ordinary thin cover-glass substituted.

The Secretary announced that invitations had been received from the Croydon Microscopical Society and the South London Microscopical and Natural History Club to any of the members who might wish to exhibit at their approaching soirées.

The names of several gentlemen for election at the next meeting were read.

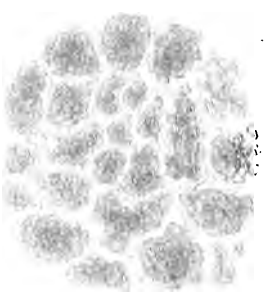
The next meeting was announced to take place on December 19th, at eight o'clock, when the nomination of officers for the ensuing year would take place, and the proceedings terminated.

Dr. Bruce then exhibited his specimens illustrative of inflammation.

The Anniversary Meeting will take place January 16, 1874.

Fig 3.

Fig 4.



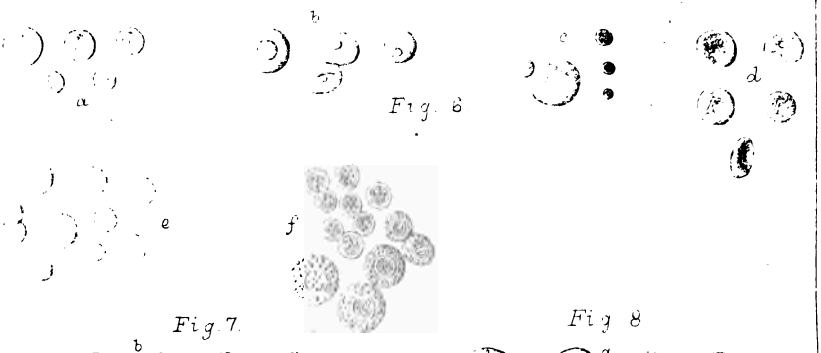



Fig. 6

Fig 7.

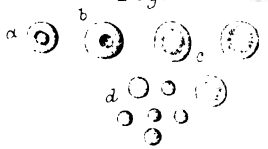



Fig 8

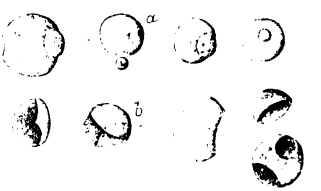


Fig 9.

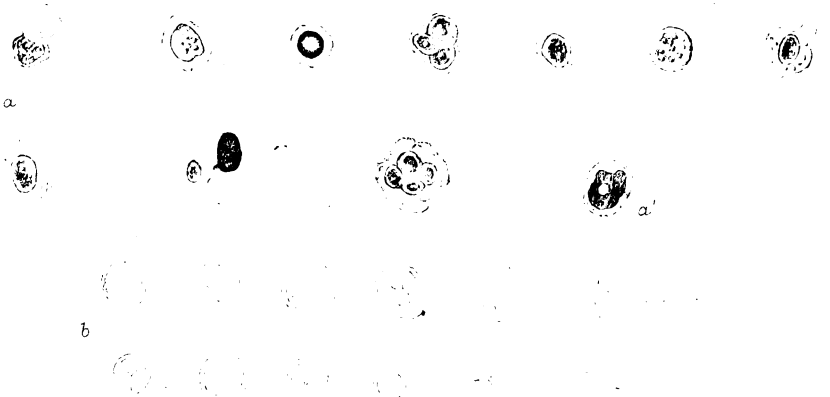



Fig 10.

Development of blood corpuscle

THE

# MONTHLY MICROSCOPICAL JOURNAL.

FEBRUARY 1, 1874.

## I.—*On the Origin and Development of the Coloured Blood Corpuscles in Man.*

By Dr. H. D. SCHMIDT, New Orleans.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Jan.* 7, 1874.)

PLATES XLIX., L., AND LOWER PART OF LI.

IN the course of the last four years, I directed my attention, in connection with a series of investigations into the histology of the nervous tissues, also to their development. In order to gain as much information as possible from the material which I made use of, consisting of a considerable number of human embryos of all stages of development, I concluded, at the same time, to pay some attention to the development of the coloured blood corpuscles, and also to that of the smaller blood-vessels. The investigations, relating to the

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EXPLANATION OF PLATES XLIX., L., AND LOWER PART OF LI.

[The Figures are correct, but the artist has unhappily not placed them in serial order on the Plates.]

FIG. 1.—Small human ovum; natural size. The specimen, having been laid open by several incisions, and emptied of its liquid contents, is here represented as it appeared under water. Being flattened by its own weight, its diameter has become considerably greater than it was previous to its being opened.

FIG. 2.—Various forms of coloured mother-blood corpuscles, which escaped from the canals of the umbilical vesicle, showing the embryo-corpuscles in their substance, as well as the concave depressions on their surface. As they are represented such as they appear, as illuminated with oblique light by means of the achromatic prism of *Abraham*, the embryo-corpuscles, owing to the transparency of the mother-corpuscle, seem to project from the surface of the latter, which, however, is in reality not the case. Magnified 465 diam.

FIG. 3.—View of the interior of a primary follicle of the umbilical vesicle, still containing a considerable number of young coloured blood corpuscles in different stages of their growth, as well as a number of mother-blood corpuscles. A number of blood corpuscles are seen to pass from the follicle into a canal, situated between it and a neighbouring follicle; a portion of the latter is seen at the margin of the drawing. At the borders of the follicles, the delicate fibrous tissue surrounding them is brought into focus, together with the faint outlines of the hexagonal cells of the upper layer. For the sake of illustration, the latter as well as the fibrous tissue are represented a little more distinct than they really appeared when the lower layer of cells and the blood corpuscles were in focus. Within the cells, the large nuclei as well as the small pale ones may be observed. The whole object is represented as illuminated with oblique light. Magnified 279 diam.

FIG. 4.—Small portion of the wall of the umbilical vesicle, situated between

blood corpuscles, therefore, form the subject treated of in the following pages.

In the beginning, my examinations, concerning the development of the coloured blood corpuscles, were confined to embryos from six to ten weeks old, in which the circulatory apparatus, judging from the degree of its development, must have been already in full activity,—and accordingly, the period of the original production of these bodies had passed. For this reason I gained no further information relating to their origin, but became only acquainted with the process of their multiplication. Chance, however, threw into my hands, while I was still engaged in these researches, a human ovum, one half an hour after its expulsion, the diameter of which, including the villi of the chorion, did not exceed 2½ ctms. This specimen afforded me not only an opportunity to become acquainted with the origin of these blood corpuscles, but disclosed to me also a certain process of multiplication of nuclei, which, as far as I am aware, has not heretofore been observed in the tissues of vertebrated animals.

When I first saw this ovum, it was enveloped, with the exception of about one-third of its surface, by a clot of blood. On the uncovered portion, an area of about 1½ ctm. in diameter was found,

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the amnion and chorion of the human embryo, 16 mm. in length, mentioned in the text. Magnified 349 diam.

Fig. 5.—Portion of the wall of the umbilical vesicle of the small human ovum, showing the grouping of the follicles; the light portion between the latter represents the canals. Magnified 49 diam.

Fig. 6.—Blood corpuscles from the spleen of a human embryo, about twelve weeks old;—*a*, mature coloured blood corpuscles; *b*, mother-corpuscles, containing but one embryo-corpuscle; *c*, an embryo-corpuscle, shortly after its detachment from the mother-substance; also some young coloured corpuscles of different sizes; *d*, mother-corpuscles, showing small openings or slits on their surfaces; also another with a large depression, laying on its side; *e*, group of very pale disk-shaped corpuscles, probably derived from colourless blood corpuscles; *f*, groups of free nuclei and colourless blood corpuscles. Magnified 465 diam.; illuminated with oblique light.

Fig. 7.—Coloured blood corpuscles from the spleen of a human embryo, about four months old; *a* and *b*, mother-corpuscles; *c*, mature; *d*, undeveloped with biconvex surfaces. Magnified 465 diam.; illuminated with oblique light.

Fig. 8.—Blood corpuscles of the spleen of a human embryo, about four and a half months old; *a*, mother-blood corpuscles; *b*, remains of old mother-blood corpuscles; *c*, nucleated cells; *d*, pale disk-shaped corpuscles. Magnified 720 diam.; illuminated with oblique light.

Fig. 9.—Elements of the blood, taken from the right auricle of the heart of a human fœtus, about five and a half months old; *a*, probably the remains of colourless blood corpuscles, their nuclei being metamorphosed into coloured corpuscles; *a'*, similar element with a blood crystal in its centre, met with in the blood of the spleen; *b*, remains of coloured mother-corpuscles, acted on by chromic acid solution. Magnified 720 diam.

Fig. 10.—*a*, singular clear, double-contoured cell, attached to a pus corpuscle and containing a coloured blood corpuscle, met with in a specimen of urine containing pus. The enclosed blood corpuscle was seen to turn around its own axis, as described in the text; *b*, the corpuscle presenting its side, showing its cup-shaped form. Magnified 465 diam.

from which the villi had been torn, their roots still remaining. This part had evidently been in a more intimate connection with the membrane decidua than the other portions of the surface. Two hours after the abortion, the ovum being carefully freed from the coagulated blood, I opened it, and allowed a part of the very clear serous liquid, contained within, to escape. The first thing which presented itself to view was a small balloon-shaped vesicle (umbilical vesicle?) of about 6 mm. in breadth by 9 in length, showing numerous, slightly elevated, red spots upon its surface (see Fig. 1 and explanation). An embryo, corresponding to the size of the ovum, such as I had reason to expect, was not to be found. A subsequent examination, however, disclosed that the vesicle was not only connected to the inner surface of the ovum by a small pedicle, but also to a certain opaque mass situated in the wall of the ovum, and which proved to consist of a conglomeration of certain cells and nuclei, to be hereafter described.

There can remain no doubt that this small mass of cells and nuclei represented the rudimentary embryo, especially as a certain arrangement of these elements, as we shall see hereafter, could not be mistaken, and as furthermore a great number of them were undergoing a process of multiplication. In comparing this accumulation of cells, however, with other parts of the ovum, much farther advanced in their histological development, it appeared as if it represented an embryo arrested in its development. Interesting as this phenomenon may be, concerning the science of embryology, I shall nevertheless hesitate to conjecture about it, but confine myself here to the statement, that the woman who aborted the ovum, although not having menstruated for three months, had not felt the usual symptoms of pregnancy but three weeks previous to the abortion.

In order not to lose the opportunity of examining the tissues in their fresh state, and in their natural liquid, I removed at once a small piece of the wall of the vesicle with a pair of fine scissors, and prepared it for microscopical examination. The latter showed me a great number of coloured blood corpuscles in all stages of development, moving through smaller or larger canals, or issuing from the orifices of the latter which had been produced by the cut of the scissors. This movement, caused by the pressure of the covering glass, as well as by the ensuing issue of a considerable number of blood corpuscles from the cut orifices of the canals, showed me that these latter communicated with each other in the form of a network. An exceedingly fine fibrous tissue could be recognized on their thin transparent walls. In fact, it was only through the movement of the blood corpuscles that I became enabled to discern the canals, in consequence of which it became impossible to study the mutual arrangement of the latter somewhat closer, or to sketch them, for, as soon as a certain portion of the blood had

escaped, the movement of the corpuscles naturally ceased. To this circumstance it may be ascribed, too, that in a second piece, which I also removed with the scissors, the movement only slowly took place, and lasted but a short time; for, at this time, the canals of the entire wall of the vesicle, as well as those of the small piece removed, had lost a part of their blood corpuscles through the orifices caused by the section. There was a difference in the diameters of the canals; for, while some were not larger than a capillary vessel, others had attained a diameter double or three times greater. In some of the larger ones, the blood corpuscles were so densely crowded that they assumed, by mutual pressure, a temporarily hexagonal form. Besides those blood corpuscles, contained in the canals, a great number of others were observed, which, accumulated without any special arrangement in round or oblong masses, were also in motion. It was these accumulations that caused the above-mentioned red spots upon the outer surface of the vesicle. A considerable portion of these blood corpuscles remained in their places after the evacuation of the canals. The real tissue of the vesicle was composed of very large and clear hexagonal cells, containing a large round nucleus. These proved to be, as we shall hereafter see, the primary organs of origin of the coloured blood corpuscles. Before we can proceed, however, to give a special description of these cells, as well as of their arrangement, it will first be necessary to examine a little closer the embryonic blood corpuscles themselves.

Those bodies, which escaped through the cut orifices of the canals (Fig. 2), corresponded in general to the fully-developed coloured blood corpuscles of man or other mammalia; they only differed from each other in size. They were of the usual yellowish tint, entirely homogeneous in composition, soft, elastic, and round. No trace of the existence of a membrane could be discovered in their fresh and unchanged condition. The greater portion of them were of the size of the fully-developed human corpuscles, and differed from these in no way, excepting that the central depression was either wanting or but slightly marked. The whole corpuscle resembled rather a flattened disk with a rounded margin. Another portion, embracing the larger specimens, consisted of breeding or mother corpuscles, the several diameters of which ranged from $\frac{5}{600}$ to $\frac{8}{300}$ mm., or even more. These bodies contained within their substance embryo-blood corpuscles, and many of them furthermore distinguished themselves from other blood corpuscles by certain regularly-formed concave depressions on their surface, corresponding to the segment of a sphere, and indicating the place where the young corpuscle had been detached from the mother-body (Fig. 2). While the larger of these mother-bodies contained from three to four embryo-corpuscles, the smaller ones usually contained but one.

So far as I am able to judge from careful examination of these bodies, as well as of others taken from older human embryos, their process of multiplication consists therein, that in the substance of the mother-body, and very near its surface, the separation of a small portion, globular in form, takes place, which represents the embryo-blood corpuscle. Enlarging at the expense of the mother-substance, this makes its way to the surface, and, finally detaching itself, leaves behind a concave depression corresponding to its form.

Judging from the number of depressions present on many mother-corpuscles, as well as from the young blood contained within them, this process appeared to have repeated itself from three to four times in the same body. Their reproductive power, however, did not always seem to be in a constant proportion to their size, as, in some instances, the smaller ones showed as many depressions as the larger. This view was confirmed by the fact, that, while many of the larger bodies contained, in addition to the depressions, from three to four embryo-corpuscles, others of the same size gave no evidence either of depressions or blood. In some instances I found three generations represented in one body, the young corpuscle bearing within its substance another embryo-corpuscle, even prior to its own birth. In these cases, however, the mother-corpuscles contained but one or at most two embryo-corpuscles, and only one of these contained the third generation in the form of a small globule. The reproductive force appeared here to have been concentrated at one point. In some cases, where several embryo-corpuscles were contained within one mother-body, they were sometimes situated opposite to each other, having the appearance of having been formed by the division of one body. A change of focus, however, showed this not to be the case.

As can be seen from the above, the observations thus far described, concerning the process of multiplication of the coloured blood corpuscles, do not correspond to those of other observers. The prevailing theory on this subject is, to the extent of my knowledge, still that whereby the multiplication takes place by a division of nucleated coloured corpuscles. It rests mainly upon the statements of Remak, Kölliker, and other investigators, whose observations were made on different lower and higher vertebrated animals. Whether the process of multiplication of these bodies in the human embryo really differs from that in other vertebrata, or whether this discrepancy is dependent upon other causes, future investigations will decide.

Those nucleated coloured blood corpuscles which have been noticed by other observers before me to occur up to a certain period of embryonic life, correspond to those mother-blood corpuscles above described, containing but one embryo-corpuscle. These represent in the blood of the human embryo of six or seven

weeks, as we shall hereafter see, the only remaining bodies of this kind. What those investigators regarded as a nucleus, by the division of which the process of multiplication is said to take place, I have designated above as an embryo-blood corpuscle. I have preferred this expression because the young blood corpuscle arises not, as is the case in the ordinary endogenous process of multiplication of nuclei and cells, in the form of a globule or vesicle from a liquid protoplasm, but separates, as it appears, directly from the substance of the mother-corpuscle, and possesses from the beginning all the properties of the latter, even that of becoming crenated. In Fig. 2 such a specimen is represented, which, being still enclosed in the mother-substance, already contains an embryo of its own.

The mode of multiplication of the coloured blood corpuscle of man is, accordingly, one of its own kind, and has, as far as I know, hitherto not been observed. It represents, so to say, the transition from the process of multiplication by endogenous formation, to that of budding, or gemmation. As regards the occurrence of the latter in the tissues of vertebrated animals, I have already observed it, during my researches upon the development of the nervous tissues, to take place in the tissues of older human embryos. I, however, at that time, was not able to understand it in all its bearings, until I observed it again in the membranes of the small ovum above described. Here I had an opportunity to see the whole process going on, affecting the multiplication of the nuclei, as well as that of certain cells arising from a portion of the latter, and destined to the formation of embryonic blood-vessels. The description of this mode of multiplication I must, however, pass over in this place, as it would lead me off too far from my original subject, the origin and development of the blood corpuscles. I shall, therefore, discuss it more minutely in another paper, treating of the formation and development of the embryonic blood-vessels.

Before pursuing the process of multiplication of the coloured blood corpuscles any farther through the successive stages of embryonic life, we will first examine their origin a little closer. The observations thus far described were made, as already mentioned, on the fresh specimens examined in the serous liquid of the ovum. At their conclusion, the approaching darkness of the evening interrupted my labours, and it was necessary to put the ovum in a weak solution of chromic acid for preservation. In resuming the investigation on the following day, I examined again a little piece of the balloon-shaped vesicle, but now received another aspect of the object. The canals, namely, had been almost entirely emptied of their blood corpuscles, and were, in consequence, not so easily to be recognized as such, as before. The accumulations of coloured blood corpuscles, however, had, though not in their original bulk, remained behind. Although this circumstance de-

prived me of the opportunity of studying the details of that system of canals somewhat closer, it, nevertheless, enabled me better to understand the true signification of those large hexagonal cells. I discovered, namely, that those accumulations of blood corpuscles were situated between two membrane-like layers of these cells, and that I had in reality before me a system of *primary glandular follicles* (see Fig. 3). The latter themselves were very irregular in size and form, and appeared somewhat flatly pressed, which, however, may be ascribed to the emptying of their blood corpuscles, as well as to the delicacy of the tissue yielding to the weight of the covering glass. On examination with an amplification of 65 diameters, the whole appeared as a close group of islands, among which the canals could be seen in the form of clear stripes passing between the opaque follicles (Fig. 5). A higher amplification showed that the principal elements of the latter, the large cells, distinguished themselves by a more or less hexagonal form, as well as by a fine, sharply-defined, double contour, and contained, besides a large round double-contoured nucleus, also a number of smaller ones. These latter were pale bodies, also bordered by a double contour, and represented, as we shall see below, the successive stages of the large nucleus. Two to three of these small nuclei were also observed in the interior of the large and fully-developed nuclei. Some of the cells even contained two of these latter, one of which, however, was always smaller than the other. The protoplasm filling up the interior of the cells was finely granular. The diameter of the cells ranged from $\frac{1}{300}$ to $\frac{1}{300}$ mm.; that of the large mother-nuclei from $\frac{8}{300}$ to $\frac{8}{300}$ mm. The small free nuclei contained within the cells measured from $\frac{4}{800}$ to $\frac{8}{800}$ mm., while the diameter of those contained within the large nuclei never exceeded $\frac{4}{800}$ mm. The walls of the canals occupying the interspaces of the follicles consisted of a fine fibrous tissue, which, in the form of an exceedingly delicate membrane, extended itself over the former to perform the function of a supporting tissue (see Fig. 3 and explanation).

In considering the construction of these large hexagonal cells, as well as their arrangement in the form of a follicle, a little closer, we shall find that a certain relationship between the nuclei contained within them, and those large, above described, mother blood corpuscles, cannot be overlooked. On the contrary, those pale, double-contoured bodies within the cells, seem to represent the successive stages of development of the larger mother-nuclei, while these latter are most likely identical with the breeding or mother corpuscles. The only difference which could possibly be discovered would consist in the presence of a double contour, and in the want of colour in the nuclei contained within the cells. But as the double contour itself already indicates the existence of an enveloping membrane,

I must, for the sake of explanation as to the cause of this difference, allow myself to offer some remarks regarding the behaviour of fully-developed blood corpuscles in reference to external influences. These also involve, to some extent, the question regarding the existence or non-existence of an enveloping membrane of these bodies.

In examining fresh and fully-developed human blood corpuscles in their natural liquid, the liquor sanguinis, it is impossible to discover a distinct trace of an enveloping membrane upon them; but, in treating them with pure water, they soon become colourless by parting with their colouring matter, and apparently disappear from view. By a still closer examination, however, and with a first-class objective, they are to be seen in the form of very delicate small vesicles, bounded by a fine double contour. By the escape of their contents through the endosmosis of the water they have somewhat shrunken, and, in consequence, become smaller in diameter. Some of them have lost their biconcavity, and been rendered biconvex, or even spherical, while others have retained the shape they possessed before the application of the water, either biconcave, cup-shaped, or even crenated. All, however, without regard to their form, show the delicate double contour. In this condition the blood corpuscles are much inclined to congregate in groups adhering to each other. In some instances where a number of corpuscles have, by mutual pressure, assumed a hexagonal form, as it frequently occurs, this is retained after the treatment with water, and, by virtue of the double contour which they now show, the whole group appears almost as a layer of pavement-epithelium. More rapidly still than water, acts a weak solution of chromic acid. As soon as a drop of this is brought under the covering glass, the double contour appears, and sharper even than in the preceding case. If the solution is sufficiently strong, it then coagulates the liquor sanguinis in a granular and fibrillar form. In this case the blood corpuscles lose their own colour, but appear tinted by the chromic acid. If the solution is very weak, they will then appear in the form of well-defined and clear double-contoured vesicles. Treated with a solution of chromic acid, they lose but little of their diameter. When coloured blood corpuscles are exposed for some time to the action of the gastric juice—as it occurs in yellow fever, where considerable extravasations from the smaller blood-vessels of the stomach take place, to be finally ejected in the form of black vomit—the same changes, with a still greater loss of diameter, are observed.

The behaviour of coloured blood corpuscles in relation to external influences is not always the same during different periods of their development and course of life. Thus, for instance, are the larger and older corpuscles of human embryos changed into

clear and sharply-defined double-contoured cells by a very weak solution of chromic acid, while the younger specimens remain totally unaffected. Among the small veins of human embryos that have been lying in such a solution, many examples are met with, in the interior of which the coloured blood corpuscles, arrested and accumulated by the death of the embryo, have assumed an hexagonal form, and appear, in consequence of the action of the chromic acid solution, here as clear double-contoured hexagonal cells; they may thus give rise to serious errors. Even in the blood of adults a number of coloured corpuscles are frequently observed, which resist more or less the influence of one or the other reagent. This fact, in connection with those above mentioned, seems to indicate that the chemical composition of these bodies is not the same in all periods of their life.

These observations show that, under certain circumstances, the coloured blood corpuscles may appear in the form of double-contoured cells. The question arises, therefore, whether this double contour is an artificial production, or whether it really represents a distinct part of the fresh blood corpuscle. In those cases where the reagents used were acids, it may be alleged that, while by their action the superficial portion of the blood corpuscle, to a certain depth, was rendered more dense, the water of the solution, at the same time, dissolved the inner portion, resulting in the production of an artificial double-contoured cell. This explanation, however, could not be applied to those cases where an entirely neutral agent, as water, was used. The double contour, therefore, must evidently depend upon some other cause. Might it not be that the coloured blood corpuscle of man, when it arrives at maturity, undergoes a slight condensation on its surface, in the form of a thin layer or pellicle, which, resisting the solving power of the water, would finally appear in the form of a double contour. This explanation has been, if I remember rightly, advanced before, and seems to be more plausible than to deny to the blood corpuscle the existence of an enveloping membrane altogether. It is, moreover, supported by the fact which I have stated above, namely, that the younger embryonic blood corpuscles remain unaffected when coming in contact with a weak solution of chromic acid, while on the older ones a double contour makes its appearance. There are other facts which I have observed in support of the existence of an enveloping membrane on the human blood corpuscle, the statement of which I must, however, postpone to a later period. I beg to state here that, regarding this subject, I have, in the course of this year, made a series of close examinations on those giant blood corpuscles of *Amphiuma means* [? *Astridactylum*, Ed.] vulgarly called "conger eel," and also on the frog, the results of which indicate the existence of a membrane. As, however, the

multiplication and development of the nucleated corpuscles of these animals takes place by a process different from that in the human embryo, we are not justified in supposing that both kinds of blood corpuscles must, necessarily, resemble each other in structure. The details of these researches will form the subject of another treatise. We will now return to the birth-place of the primary human blood corpuscles, and try to prove the identity of those large nuclei, contained within the hexagonal cells, with those mother-blood corpuscles before described.

The examination of the blood corpuscles, escaping through the cut orifices of the system of canals in the wall of the vesicle, in their fresh and unchanged condition, claimed at first, as mentioned above, the greater part of my attention. The large hexagonal cells I only passingly observed at the margin of the preparation. The larger portion of them, it is true, were hidden from view by those accumulations of blood corpuscles, and, although recognizable by change of focus, no particular attention was paid to their examination at this time. It was, therefore, not until the following day, the preparation having in the meantime been subjected to the action of the solution of chromic acid, and a considerable portion of the blood corpuscles having escaped from the follicles, as well as from the canals, that I recognized the true nature of these cells. As far as I could remember from the previous examination of the fresh specimen, the large nuclei, contained within the latter, were not coloured and showed a double contour. But, as the greater portion of them were obscured by the blood corpuscles, it is not impossible that they existed here in different stages of development, and that a number of them were already more or less provided with the characteristic colouring matter of the blood. Had this really been the case, they would, nevertheless, subsequently, like the large mother-blood corpuscles, have become discoloured by the chromic acid solution, and, consequently, have appeared with a double contour.

To convince myself more fully of the correctness of my view, I examined the wall of the umbilical vesicle, situated between the amnion and chorion of a human embryo, 16 mm. in length. The embryo was one of the best specimens on which I had worked, as it came into my possession only a few hours after its abortion, in a very fresh and, as it appeared, in every respect normal condition; and although it had at the time of this examination remained several months in a weak solution of chromic acid, still it nevertheless showed sufficiently the structure of its umbilical vesicle. The wall of the latter proved to be a membrane consisting of a delicate fibrous tissue, the interior surface of which served as a base to a tolerably thick layer of those large hexagonal cells. These appeared somewhat shrivelled, granular and of a greenish yellow tint, but although still hexagonal in form, they had nevertheless lost the well-defined

regular double contour. These changes may probably be attributed to the action of the chromic acid. Still, the larger and smaller nuclei could be easily distinguished in the interior of the cells, and moreover, many of the former were observed upon the surface of the membrane, together with a number of blood corpuscles which manifested themselves by their greenish glimmering appearance (Fig. 4). This last examination demonstrated that the anatomical structure of both vesicles in question, was one and the same,—and as there remains no doubt that, in the latter instance, I really examined the umbilical vesicle, one might reasonably suppose that the balloon-like vesicle of the younger ovum—notwithstanding the anomaly of its embryo—represented the same embryonic organ.

Although further examinations made of similar and fresh specimens are absolutely necessary for a final decision, the facts already observed, and being in accord with each other, strongly indicate that the primary birth-place of the coloured blood corpuscles in the human embryo, is to be sought in the above-described gland-like follicles of the umbilical vesicle.

The origin of these blood corpuscles, according to an older theory, from the axial cells of the embryonic heart and the larger blood-vessels, appears to me as improbable, as their derivation from the axis of certain columns, composed of embryonic cells, and destined to the formation of capillaries. It is more natural to suppose that the formation of the first coloured blood corpuscles in the embryo occurs, similarly to that of the colourless ones, in the adult, by the medium of certain glandular organs, adapted to this purpose. This view is furthermore confirmed by the established fact, that at a later period of life the former are derived from the latter. According to my own observations, this already takes place during embryonic life. Neither do the observations, made in the last few years by *Klein* on the yolk-sac of the chick,* according to which the blood corpuscles originate from cells, arising and separating from the interior wall of certain vesicles that are destined to be afterwards converted into blood-vessels, correspond with the above-described facts, observed by me. The more recent investigations of *Balfour*,† however, regarding the origin of the blood corpuscles and blood-vessels of the chick, seem almost to indicate that the process of formation and multiplication of these bodies in the embryo of birds differs somewhat from that in man. His statements, confirming, to some extent, those of *Klein*, correspond very nearly to one of the earlier theories, according to which the blood-vessels in the chick were formed by the fusion of certain stellate cells, and the blood corpuscles were derived from the nuclei of these cells.

* Stricker, 'Handbuch der Lehre von den Geweben des Menschen,' xx., p. 1219.

† 'Quarterly Journal of Microscopical Science,' July, 1873, p. 280.

It has already been mentioned that the balloon-like vesicle was connected to the rudimental embryo by a fine pedicle (Fig. 1). Upon examination of the latter in water, and with a low magnifying power, two thread-like shades were observed to pass through it from the vesicle to the embryo, but which became so pale by an addition of glycerine, as to show no particular character, when examined with a higher amplification. At their entrance in the embryo, however, they could be observed to communicate with an opaque network, from which observation I supposed that the whole was a continuation of the system of canals of the vesicle. This supposition was afterwards confirmed by a closer examination of small fragments of the embryo, in which I observed some of these small canals, containing blood corpuscles. The embryo itself consisted of fine granular fibrillæ, arranged in plexus-like bundles, the interspaces being filled with a considerable number of embryonic nuclei and numerous granules. Besides these elements a number of large hexagonal cells, and certain free nuclei were still observed on the margin of the embryo. This whole mass of fibrillæ, granules, nuclei, &c., &c., representing the rudimentary embryo, was situated between the two layers of the ovum. The interesting results of a careful examination of the latter with their appended villi, as well as of the process of multiplication of the nuclei, met with in this ovum, I must forego for the present; in my treatise on the formation and development of the embryonic blood-vessels, I shall discuss them more in detail.

Whether this embryonic mass of fibrils, granules, nuclei, &c., &c., represented the remains of an embryo, stunted in its growth, or only a single part of it thus far developed, it would be difficult to decide. As I had hitherto no opportunity of examining practically of my own accord the ovum in its earliest stages of development, I feel some delicacy in attempting to give a solution of the phenomenon, and I am all the more anxious to learn the views of more experienced embryologists upon this subject. According to the best of my judgment, it had not been arrested in its growth. It might rather be supposed that the material, forming the *area germinativa*, did not suffice for the formation of the entire embryo, in consequence of which only a part of it had been formed. But as the umbilical vesicle especially, was so fully developed, it is not impossible that the above-described mass of fibrils, nuclei, &c., &c., should represent the only partially-formed alimentary canal, and that, accordingly, the *vegetative layer* alone had taken part in the formative process.* We will now return to the embryonic blood

* I was led to take this view of the subject by the examination of a human ovum of five weeks (according to the statement of the woman), in which not the slightest trace of an embryo was to be found, although the appended villi were well developed in form. The structure of the walls of this ovum, as well as of its villi, consisted of a delicate fibrous tissue containing a great number of oval

corpuscles, and pursue their formation further through the different stages of embryonic life.

It will be remembered, that among those blood corpuscles issuing from the orifices of the canals, there were some which contained besides those concave depressions—"traces of a preceding generation"—still a younger brood of three to four embryo-corpuscles within their substance (Fig. 2). This fact indicates that the process of multiplication of these bodies in this early period of embryonic life must take place quite rapidly. Another fact, worthy of notice, was the entire absence of colourless blood corpuscles, but which is fully explained by the absence of those organs, *viz.* spleen and lymphatic glands, which, at a later period, are concerned in their formation. In fact, those colourless bodies only appear simultaneously with the development of the latter-named organs, and at a time when the primary endogenous process of multiplication has become much slower in its course, eventually ceasing almost entirely.

With some embryos of 16 to 20 mm. in length, which I examined, I had no opportunity to examine the blood in its fresh condition, either because the examinations of the nervous tissues first claimed my attention, or that, for want of time, I was compelled to put the specimen, for the sake of preservation, in a weak solution of chromic acid. At a subsequent examination of the pia mater of the spinal marrow, however, I found that the blood left behind in the vessels contained no more of those large mother-blood corpuscles, enclosing three or more embryo-corpuscles. Although I still met with a number of bodies, showing two to three concave depressions, no more than one embryo-corpuscle was observed within their substance. Their diameter, also, had considerably diminished. The greater portion of the blood consisted here already of fully-developed corpuscles.

In embryos of eight to nine weeks I first had the opportunity of making a close examination of the coloured blood corpuscles in their fresh condition. Judging from the advanced development of the heart, and especially of the smaller blood-vessels, the circulation of the blood at this period must already take place quite regularly. By far the greater majority of the blood corpuscles are fully developed. The large mother-corpuscles, containing several embryos, have disappeared, but a considerable number of those smaller ones, enclosing only one embryo-corpuscle, are still there. The process of multiplication accordingly occurs now much more slowly. A weak solution of chromic acid discolours the mother-bodies and

nuclei; its interior surface was lined by a tesselated epithelium, the hexagonal cells of which contained very clear oval nuclei. Upon this rested very loosely a delicate spider-web-like layer of fibrous tissue. No trace of the formation of embryonic blood-vessels was to be seen. The ovum was at its expulsion still surrounded by the entire membrana decidua.

imparts to the embryo-corpuscles within them a granular aspect, but without discolouring them. The older fully-developed blood corpuscles undergo also a discolouration by the action of this solution, in consequence of which they are seen in the interior of the smaller blood-vessels in the form of clear, double-contoured cells. The younger ones remain, with the exception of an inconsiderable diminution of their diameter, unchanged—that is, they retain their colouring matter, which assumes, by the action of the chromic acid, a greenish shining appearance; these are but seldom met with in the finer capillaries. It must here be mentioned, that the mother-blood corpuscles, in general, are naturally somewhat paler than the embryo-corpuscles which they contain, and that the loss of colour is not always due to the action of the chromic acid solution; on the contrary, I have sometimes observed clear double-contoured mother-bodies in fresh blood. Small blood crystals are sometimes seen in the interior of the smaller blood-vessels or in the embryonic blood corpuscles themselves, especially in such specimens as have remained for some time in a chromic acid solution. On the matured blood corpuscles of this period, the same changes of form are observed as occur in those of the adult. More especially are they inclined to assume a cup-shaped form by the swelling of their margins, and this takes place sometimes to such a degree that they assume the form of a hollow sphere, on which nothing more than a small orifice remains, bounded by the contracted margin. In regard to the colourless blood corpuscles, it may here be mentioned, that up to this period none of them were observed either in the heart or in the blood-vessels.

The further now the embryo advances in its development, and the more that blood-forming organ, the spleen, attains to perfection, the rarer becomes the primary endogenous formation of the coloured blood corpuscles; nevertheless a considerable number of mother-corpuscles, as we shall see directly, are still met with at the beginning of the fourth month. The greater portion of the fresh blood, taken from the heart of an embryo about twelve weeks old, consisted accordingly of fully-developed corpuscles, that is, such as were marked by a round margin and a central concavity (Fig. 6, *a*); their diameter differed considerably, and ranged from $\frac{2}{300}$ to $\frac{4}{300}$ mm. In addition to these, I observed still a number of mother-blood corpuscles, the most of which contained but one embryo-corpuscle (Fig. 6, *b*). The size of the latter, however, did not stand in due proportion to their maturity, as a number of them had already attained a diameter of $\frac{8}{600}$ mm., while, on the other hand, a number of young, already liberated corpuscles (Fig. 6, *c*), had scarcely attained one of $\frac{3}{600}$ mm.

In the same blood I also met with some young blood corpuscles, just in the act of detaching themselves from the mother-corpuscle;

though already fully detached, they had as yet not entirely left the concave depression (Fig. 6, *c*). I further observed a number of others which distinguished themselves from the mature corpuscles by their biconvex surfaces; these had very probably been detached from their mother-body not long before, and, in consequence, not yet attained the final biconcave form (Fig. 6, *c*). Finally, I observed still a number of very pale flat bodies, resembling a flat disk. The most of these were seen to have collected in groups (Fig. 6, *e*); they also represented young blood corpuscles, but seem to have been derived from another source, hereafter to be mentioned.

In examining the blood of the fresh pulp of the spleen of the same embryo, I found, in addition to these elements just described, also a considerable number of free nuclei and cells, generally collected in small masses. The former were bounded by a fine double contour. Their interior was filled with granules. The latter consisted of the same nuclei, enveloped in a granular mass, and differed in no way from the colourless blood corpuscles of the adult (Fig. 6, *f*).

One month later, in the beginning of the fifth month, the primary endogenous formation of young coloured blood corpuscles goes on still more slowly. Nevertheless, a certain number of mother-corpuscles, containing but one embryo-corpuscle, are still observed (Figs. 7 and 8, *a*). This is especially the case in the blood of the fresh pulp of the spleen. A small number of others I also observed here, which had already rid themselves of their brood, the traces of which they still bore in the form of one or two concave depressions (Fig. 8, *b*). The soft margins of the latter were kept in a flapping-like motion, when the blood corpuscle, carried by the current, glided along under the covering glass while turning upon itself. Some of these bodies still contained an embryo-corpuscle, a fact which shows that the mother-corpuscles of this period are still capable of generating two embryos, though each successively. Besides a large multitude of mature coloured blood corpuscles (Fig. 7, *c*), a considerable number of young biconvex ones (*d*) were also observed, which seemed, as before mentioned, to have been detached from the mother-bodies but a short time before. The colourless elements of the pulp of the spleen showed the same character as those of the adult spleen; they consisted of granular nuclei, nucleated cells, and fully-developed colourless blood corpuscles (Fig. 8, *c*). In the heart I met with the same elements, with the exception of the granular nuclei and cells. The colourless blood corpuscles here met with seemed to be in different stages of development, for a considerable difference existed in their diameters, some of them were even smaller than a coloured corpuscle. The greater part of them contained but one nucleus, others two. The most of these bodies were round, while the rest were irregular

in form. It is not impossible that amœboid movements had already taken place in the latter before the death of the embryo set in. A large number of coloured blood corpuscles had assumed a cup-shaped form. Although a number of young coloured corpuscles, either free or still attached to the mother-corpuscle, were observed here too, they were, however, not so numerous as in the pulp of the spleen.

In the blood of the fœtus of 5½ months, the endogenous process of formation has, finally, almost entirely ceased. The number of mother-corpuscles containing embryo-corpuscles, is here quite small. Nevertheless a number of them, discoloured and double-contoured by the action of chromic acid, and marked by concave depressions and slits, from which the embryo-corpuscles had escaped (Fig. 9, *b*), I still found in the right auricle of the heart. Besides these I also found there a number of coloured blood corpuscles not fully developed, which, although still surrounded by a colourless membrane-like envelope thrown into irregular folds, betrayed their true character by their colour (Fig. 9, *a*). In the most of these cases the envelope embraced only one of these bodies, but in some three or four. In the spleen I met with the same bodies, only in smaller numbers; in one case I observed a blood crystal in the centre of the coloured body (Fig. 9, *a*): It is very probable that these bodies represented colourless blood corpuscles, the nuclei of which were in their last stage of metamorphosis into coloured blood corpuscles. This view is corroborated by the circumstance that a number of colourless blood corpuscles were observed in company with them. The shrivelled appearance of the envelope was probably due to the action of chromic acid, in a solution of which the fœtus had been lying for several days with the thorax opened.

From the foregoing examinations, it can be seen that with the appearance and the steady increase of the colourless blood corpuscles in the blood of the human embryo, the endogenous formation of the coloured corpuscles gradually decreases. But as I did not extend these examinations beyond the last-mentioned period, I am not able to state exactly the time when this formative process ends; very probably at birth, when the embryo has arrived at full maturity, and begins its independent existence with its first inspiration. Notwithstanding, small coloured mother-blood corpuscles, containing a small embryo-corpuscle, are still met with now and then in the blood of the adult; their diameters, however, never exceed that of the matured corpuscle.

If we accept now the generation of the primary coloured blood corpuscles in the human embryo in its earliest stages of development, as taking place in certain glandular cells destined for this purpose, the question next arises, what is the first momentum which sets them in motion? According to those existing theories, already partially

mentioned, and regarding the primary formation of the coloured blood corpuscles in the interior of the embryonic blood-vessels, this naturally must proceed from the heart, which, after having attained a certain degree of development, begins its pumping action. In accordance with the theory of Remak and Kölliker, this can only take place after the axial cells of those columns destined to the formation of blood-vessels, have been loosened by the action of a secreted fluid, and converted into blood corpuscles.* But this theory is not in concert with my own observations regarding the development of the first embryonic blood-vessels within the membranes of the small human ovum, described at the beginning of this essay. Neither in the blood-vessels of this ovum, nor in the interior of those of older embryos, did I ever meet with blood corpuscles *during the primary stage of the process of formation of these vessels.* I suppose, of course, that the mode of formation of the primary blood-vessels—particularly of the smaller ones—in the embryo itself, does not differ from that in the membranes and villi of the ovum. Regarding, therefore, the explanation given by the last-named authors, as not entirely satisfactory, I shall take the liberty of pointing to another mode, in which the blood of the embryo might first be set in motion.

In the smaller blood-vessels of human embryos from eight to nine weeks old, the blood, according to my own observations, makes its way by the contractions of the heart, causing a continuous advancing of the blood corpuscles into the capillaries newly formed, but, as yet, not sufficiently open. Now the circulation of the blood within the *provisional* circulatory apparatus of the embryo, probably takes place in the same manner, only in an inverse direction; presupposed, however, that the heart and the blood-vessels of this early period are not represented any more by solid masses or columns of cells, but that, on the contrary, as I have reason to think, the former already consists of rudimentary muscular fibrillæ, and the latter of delicate, probably granular fibrillæ of fibrous tissue with numerous nuclei; further, that a communication, by simple anastomosis, between these vessels and the canals of the umbilical vesicle is already established. The continuous generation of new blood corpuscles, through the medium of the large hexagonal cells of the umbilical vesicle, necessitates an accumulation of these bodies in the interior of the follicles, as well as a distension of the walls of the latter, and, in consequence, produces a force which must eventually push the blood corpuscles from the follicles into the canals. But as the generative process goes on without interruption, the canals also become overfilled, and the blood corpuscles gradually penetrate into the interior of the embryonic vessels with which the former communicate, and thus finally enter the interior of the heart. This

* Funke, 'Lehrbuch der Physiologie,' 4. Auflage, B. II., p. 1141.

now, excited by the presence of the blood, commences its rhythmical action, and forces the blood in return through the *aorta*, the *arteriæ omphælo mesentericæ*, as well as through their communicating network of vessels, back to those blood-vessels, the *venæ terminales*, through which first it came; thus a regular circulation is established. In the consideration of this theory, it must be borne in mind that the described process is only gradual, and that the entire provisional circulatory apparatus is not completed at one and the same time. On the contrary, the circulation of the blood probably takes place at first through a few anastomoses, and is extended in proportion to the formation of new vessels.

The formation of new coloured blood corpuscles within the follicles of the umbilical vesicle probably continues until that period in which those permanent organs destined for this purpose, that is, the spleen and the lymphatic glands, are sufficiently developed to perform their functions. This did not seem to have occurred in that embryo above mentioned, of 16 mm. in length, in the umbilical vesicle of which, situated between the amnion and chorion, I still recognized very distinctly those hexagonal cells with their mother-blood corpuscles, notwithstanding the action of the chromic acid. Neither can I remember having noticed any trace of a rudimentary spleen; if any such really had been present, it must have been so small as to be easily overlooked. It might therefore be supposed that the umbilical vesicle of this period of development still represents the sole organ, generating blood corpuscles. The circulation of the blood in this case had, as yet, not extended into the villi of the chorion, for the blood-vessels of the latter were not sufficiently developed.

Some weeks later, in embryos of from eight to nine weeks old, I observed a considerable change to have taken place. The umbilical vesicle is now wasting away, but the umbilical vessels, with all their ramifications, are formed, in consequence of which the circulation of the blood through the finer blood-vessels of the chorion is established; the blood of the embryo is therefore carried within close proximity to that of the mother. The spleen and a part of the lymphatic glands are also present. The former is seen of an elongated form, similar to that of the pancreas, on the left half of the inferior margin of the stomach, attached by the peritoneum. In addition to the coloured blood corpuscles, it also contains a considerable number of those not fully developed colourless corpuscles above mentioned. But with the appearance of these organs, and the gradual wasting of the umbilical vesicle, those larger mother-blood corpuscles containing several embryo-corpuscles have disappeared; the smaller ones contain never more than one of the latter at one and the same time, but are, nevertheless, capable of producing a second one, after the first has been detached. The generative

force which was transmitted from the hexagonal cells of the umbilical vesicle to the large mother-blood corpuscles is now becoming extinct, and suffices only for the generation of one or two embryo-corpuscles. A portion of the increase of the new coloured blood corpuscles must therefore already be derived from the permanent blood-formative organs. In the embryo of twelve weeks, the spleen is found still more considerably developed. Its form is not any more oblong as before, but more contracted; it is now also further removed from the stomach, and ready to take its future permanent position. The numerous elements of the blood (Fig. 6), already described, which it contains, show sufficiently that it is now performing its function to its full extent. As far as the development of the lymphatic system is concerned, it probably stands in a certain relationship to that of the spleen and the blood-vessels in general. While, namely, a number of lymphatic glands are still in their first stages of development, others must have sufficiently advanced to assume the performance of their function.

In the preceding pages I have spoken of the spleen and the lymphatic glands as of the permanent blood-formative organs, and, in so doing, only endorsed the now prevailing opinion of other observers regarding their function. But this essay would not seem to be complete if I failed to subject to a somewhat closer consideration the formative process taking place within the organs, especially within the spleen. In addition to this, the views regarding the exact mode of the metamorphosis of the colourless into the coloured blood corpuscles, which, as it seems, still differ from one another, determine me also to add a few remarks upon the subject. The question to be answered is: Does the entire colourless blood corpuscle, or only the nucleus within it, take part in this metamorphosis?

To arrive at a satisfactory solution of this question, it is first necessary to take into a closer consideration the origin and development of these bodies. In regard to their origin there remains, according to the most recent researches of several observers concerning the structure of the spleen and lymphatic glands, but little doubt that it has to be sought in the tissue, that is, in the fibrous meshes of these organs. These meshes, extending throughout the pulp of the spleen, lodge, as is known, a considerable number of free nuclei and cells of different sizes. The former represent the greater part of these elements, and, judging by the obvious difference of their diameters, are found in different stages of development. They are bounded by a fine double contour, and contain a number of fine granules. The cells are also distinguished by a fine double contour, and contain from one to three nuclei. Besides these elements, a multitude of countless granules, as also a number of colourless and coloured blood corpuscles are observed. The latter,

however, do not seem originally to have belonged to the meshes; but, on the contrary, have carried them by manipulation, as it is impossible to remove even the smallest portion of the pulp without cutting at the same time some of the finer blood-vessels, from which these coloured blood corpuscles are derived.

The mode of multiplication of the above-named elements does not take place, according to my own observations, by a direct division of the nucleus. The process of multiplication can therefore only be explained by looking upon those nucleated cells as the birth-place of the free nuclei. These cells possess a membrane, indicated by their double contour, and can therefore not be regarded as colourless blood corpuscles. It is more probable that they represent breeding cells, and that the nuclei within them were produced by the endogenous formative process, to be eventually set free. A number of them may, after being set free, be developed into breeding cells, while the rest, at first surrounded by a thin layer of protoplasm, escape from the meshes of the pulp into the circulation of the blood, to commence their career as colourless blood corpuscles. In the course of their development the layer of protoplasm increases in thickness, and the nuclei obtain their subsequent smooth appearance by the disappearance of the granules within them. This explanation coincides also with the results of my observations on the spleen of the human embryos above mentioned, where I met with the elements just described.

In examining the colourless blood corpuscles of the circulating blood of man in their fresh condition, it is quite as impossible to discover on them an enveloping membrane as on the coloured ones. On the addition of water, however, their protoplasm becomes pale, and of a homogeneous appearance, and appears, with certain exceptions, to be bounded by a very delicate double contour. In consequence of this change, the homogeneous disk-shaped nucleus, which in some cases also shows a fine double contour, becomes more sharply defined. The colourless corpuscles of the fresh blood appear different in size and form, and even in their behaviour in relation to external influences. The differences in the size of these bodies seem to depend mainly upon the thickness of the layer of protoplasm surrounding the nucleus, in consequence of which this layer is very thin on the smaller corpuscles, which most probably represent the younger specimens. The amœboid movements observed in this protoplasm do not occur regularly in all colourless corpuscles of the same blood. In the smaller and younger ones, therefore, they are more rarely observed than in the larger and matured ones. In some cases, these movements appear immediately after the fresh drop of blood is brought upon the slide, in others, again, only after the addition of some water. Equally as indefinite is the length of time during which they are seen to occur. This noticeable difference,

which is observed in the properties of the colourless blood corpuscles, stands probably—especially in regard to their size—in a certain relation to the different stages of development in which they are found; but, in regard to the amœboid movements, they may also partially depend on the constitution of the blood itself. Thus, for instance, did I find these amœboid movements of the colourless corpuscles in the fresh blood of a boy, thirteen years old, only to begin after the addition of water, while in the blood taken from the arm of a woman, thirty years old, they manifested themselves almost immediately, and very actively. In the blood of another woman, twenty-two years of age, taken from the os uteri at the beginning of menstruation, and examined immediately, and in which the colourless blood corpuscles were but sparsely represented, and not much larger in diameter than the coloured, the movements manifested themselves immediately. In a similar manner they appeared differently in a number of other cases, sometimes even in the blood of the same individual, when it was examined at different times. The most active movements of the colourless blood corpuscles I observed in the blood of a man who fell a victim to the bite of a water-moccassin (*Triconocephalus piscivorus*). It was taken from a vein one hour and a half after death, and examined one hour later. The processes which issued from the mass of the bodies of the corpuscles presented the most variegated forms; equally as active was the dancing molecular movement of the dark-bordered granules. The addition of water affected the movements in no way. One hour later, the dancing movement of the granules was found to have ceased, while the amœboid movement of the layer of protoplasm was still going on.

Let us now ask the question: What do these amœboid movements, observed in the colourless blood corpuscles, signify? And, furthermore, do they take place while the latter are carried along by the blood in active circulation, or is an abnormal stimulus requisite for their production? The direct observations, regarding the passage of these corpuscles through the walls of the finer blood-vessels, were made on inflamed tissues, at a time when the nutrition of the latter was deranged, and when, in consequence, they were performing their general functions under abnormal conditions. The facts elicited by these observations, therefore, would not be sufficiently conclusive to presume that the movements through which the colourless blood corpuscles pass through the walls of the blood-vessels also occur while these bodies are circulating through the vessels of a tissue or organ in a normal state. Further observations made by several observers, however, have shown that some kind of organic cells really possess the capacity of passing by means of amœboid movements from one locality to another. The question whether these movements are due to an inherent property of the

cells themselves, or are only called forth under certain circumstances, and by external influences, has, as far as I could ascertain, not been decided. Nevertheless, judging from the facts thus far observed, it seems not to be impossible that the amœboid movements in the colourless blood corpuscles should also occur in the circulating blood under physiological conditions; for it would be difficult to prove that these bodies were only endowed with this power of changing their form for the sole purpose of escaping from the vessels during the process of inflammation, either to serve subsequently for some other formation process, or to be eventually thrown off by the organism in the form of pus.

According to my own observation it is principally the larger, and therefore older colourless blood corpuscles, which manifest the most active movements. The nucleus, which becomes during these movements alternately uncovered and enveloped again, remains, as it appears, in a passive condition. In some cases, I observed it even entirely uncovered, adhering only to the margin of the protoplasm in motion, and actually dragged along by this for a little distance. The nucleus is, as already mentioned, perfectly round, smooth, and almost flat; in some cases even already of a somewhat pale-yellowish tint. It therefore already possesses, including the diameter, all the characteristics of an embryonic coloured blood corpuscle, and corresponds exactly to a portion of those bodies above described, which I observed in the blood of human embryos.

Looking upon these facts from a general point of view, it appears more than probable that in the blood of man, at least, only the nuclei of the colourless blood corpuscles are concerned in the metamorphosis into coloured corpuscles. In the blood of amphibia, this seems, as I shall hereafter demonstrate in another treatise, not to be the case.

As regards the amœboid movements of the surrounding layer of protoplasm, their cause may perhaps be attributed to the presence of the nucleus itself, or, in other words, to that of the young coloured corpuscle approaching maturity. In a similar manner, as the matured ovum engenders the stimulus, calling forth those contractions of the uterus necessary to its expulsion, so also may the matured nucleus of the colourless blood corpuscle give rise to those amœboid movements of its enveloping layer of protoplasm, which finally cause its liberation. After the nucleus is liberated in this manner, the enveloping protoplasm undergoes a gradual disintegration. Such disintegrating masses of protoplasm are constantly met with in the circulating blood; sometimes a liberated nucleus still rests upon them, while again it is entirely absent; their extended amœboid form points to movements as having taken place prior to their death. In the blood of a man poisoned by cyanide of potassium, I observed, eight hours after death, a number of these

amœba-like masses of protoplasm without nuclei. Although their amœboid movement had ceased, the molecular one of the pale granules of the protoplasm was still very active, and continued, after the addition of water, for a considerable time. Dark-bordered granules were not present. The office of the layer of protoplasm of the colourless blood corpuscles would then be to nourish the nucleus contained within it, as well as to promote the metamorphosis of the latter into a coloured blood corpuscle. It is more difficult to assign a function to those groups of dark-bordered, dancing and oscillating granules, which are observed upon the surface of the protoplasm of many of the colourless blood corpuscles. If, however, we consider that similar granules are met with in other tissues, especially in such in which the molecular changes are particularly active, we might almost assign to them a function relating to these changes. This is the case in the ganglionic bodies of the nervous system, where they are met with in the form of accumulation of pigment granules upon the body itself, and sometimes also upon the processes. In the upper layer of the cortical substance of the cerebrum they are also observed, accumulated in the vicinity of certain groups of free nuclei, which they frequently entirely obscure. In all these places they appear in the same oblong form, and when accumulated in a mass form a yellowish spot. In the blood corpuscles, however, they are colourless.

In reviewing the process of generation and formation of the blood corpuscles of man, described in the preceding pages, in later life as well as in early embryonic life, it is obvious that the organs in which it takes place truly represent nothing else but glands. The structure of the follicles of the umbilical vesicle, in which the first embryonic blood corpuscles originate, corresponds in every respect to the simplest type of a gland. The structure of the spleen, on the other hand, seems, at a superficial glance, not to correspond at once to such a type. But if we imagine the fibrous layer which supports the gland cells to be converted into a reticulated fibrous structure with wide meshes—as is found in the spleen—and look upon the free nuclei and cells laying in the interspaces of this structure as gland cells, the comparison, even here, will not be difficult. We can therefore look upon the blood corpuscle as a gland cell. Even so, as the gland cells transform in their interior certain materials, derived from the liquor sanguinis, into other substances destined to subserve various purposes in the organism, just so is it the office of the blood corpuscles to promote within themselves the metamorphosis of certain elements. The difference between the two would only consist therein, that, while the gland cell in general discharges its secretion upon the surface of a membrane, *the matured blood corpuscle gives back directly to the liquor sanguinis, by its final dissolution, its secretion, consisting of its own*

*body*. A fitting analogy to the process of development of the blood corpuscles is found in that of the spermatozoa, the origin and development of which occurs within the cells of the seminal tubules. Not only in this, but also in the difference which exists in the form and size of these bodies in different classes and orders of animals, a striking analogy is to be found.

In conclusion, I shall still mention a phenomenon which I accidentally observed, and which, peculiar as it is, will only corroborate to a certain extent my views regarding the metamorphosis of the colourless into the coloured blood corpuscles. In examining the urine of a man suffering from chronic inflammation of the kidneys, I met in the sediment—consisting mainly of pus mixed with a little blood—with a clear double-contoured cell, attached to a pus corpuscle (colourless blood corpuscle?) (Fig. 10). Within this cell I observed a small coloured blood corpuscle which had assumed a cup-shape form, and which, for a short time, turned around its own axis. Evidently this cell represented a colourless blood corpuscle, which had accidentally adhered to the pus corpuscle, or perhaps had sprung from it, in which case the body, as a whole, would have represented a colourless blood corpuscle. It is difficult, however to give a satisfactory explanation of this phenomenon.

The Life-history of monads

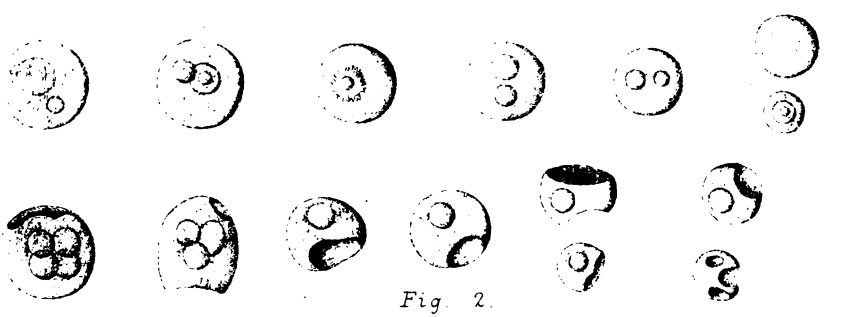


Fig. 2.

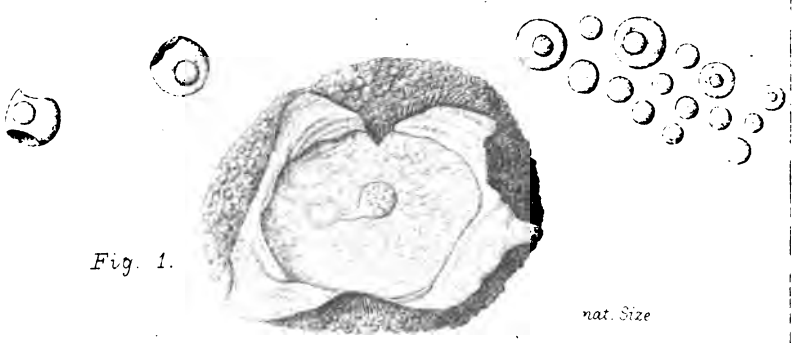
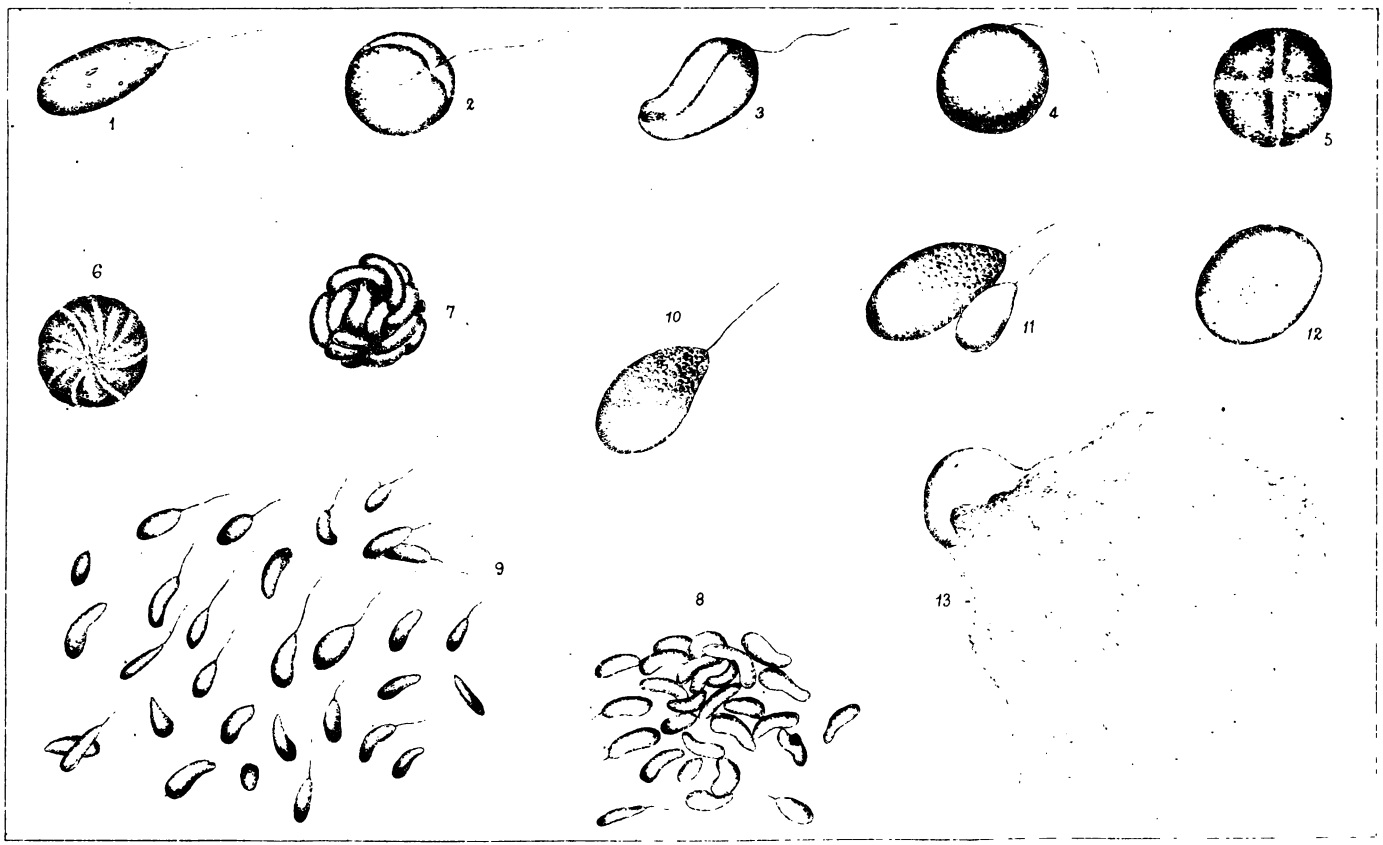


Fig. 1.

nat. Size

W. West & Co. lith.

Development of blood-corpuscle.




The Life-history of monads.

## II.—*Further Researches into the Life History of the Monads.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.

(*Read before the* Royal Microscopical Society, *Jan.* 7, 1874.)

Plate LII. and upper part of LI.

We now proceed to describe the development of the last of the monads included in the present series of researches.

Its exterior form is extremely simple, being ovoid with a single flagellum. In long diameter it never exceeds the $\frac{1}{4000}$th of an inch, and in a great number of cases was not greater than the $\frac{1}{4500}$th. Its general appearance is shown by Fig. 1, Pl. LII. The sarcode is somewhat whiter than in the others we have described, and it is much freer from vacuoles. No definite body answering to a nucleus could be made out at any stage of development, but the morphological cycle has points strikingly kindred with those before described, and at the same time points remarkably divergent.

Its mode of locomotion differs from the other forms we have described in being *quite uniform*—a slow straight motion, without jerking or irregularity. In the younger forms the flagellum was motile as usual from end to end; but in the older forms there was a greater or less thickening of the flagellum at the end attached to the body, and this in many cases became extremely marked; assuming, finally, a rigid condition for about a fourth of its length, as may be seen in the drawing, Fig. 19, Pl. LI., upper portion.

The rapidity and copiousness of its multiplication was remarked by us at a very early stage of our investigations. Multitudes of adult forms in a very short time—a time entirely too short for their development from sporules, if our former experiences were to guide us—swarmed the field. But this was subsequently explained by their remarkable mode of *multiple* fission. The process was simple and rapid.

The first indication that it was about to happen was given by the monad assuming a rounder form, sometimes as in Fig. 2, Pl. LII., slightly flattened, giving an appearance approximating to a split pea. This might continue for twenty or thirty minutes, the monad swimming freely and gracefully as before the change occurred. After this it became slightly amœboid and somewhat uncertain in form; Fig. 3 represents it as frequently seen in this stage, which was one of great activity, but only lasted from four to ten minutes, when an actually globular form was taken and the creature became still; the flagellum moved sluggishly for a very few minutes and then disappeared. Its appearance is drawn in Fig. 4. If now the little globe were carefully watched with a magnification of about 3000 diameters, there would appear, *per saltum*, a white cruciform mark—evidently a

depression—as drawn in Fig. 5. We observed this repeatedly with every variety of appliance, and the utmost power we could use, but we could discover no premonition of its appearance. There was a *sudden* transition from the condition drawn in Fig. 4 to that in Fig. 5. These lines now rapidly increased in number and became *curved* as seen in Fig. 6; and from this time deep indentures or incisions ensued, and an intense activity was set up all over the sarcode—a sort of interior whirling motion, which we can think of nothing better to assist us in describing than the rush of water round the interior of a hollow glass sphere on its way to the jet of a fountain. This would last for from ten to seventy minutes, when the sarcode would cease its activity, and very rapidly break up into the condition drawn at Fig. 7. There was no trace of an investing membrane; the constituent parts were related to each other simply as the two separating parts of sarcode in an ordinary fission, and they commenced a quick writhing motion upon each other like a knot of eels; and in this state they remained for a somewhat indefinite period, but usually from seven to thirty minutes, when they either one by one left the mass—free-swimming and flagellated monads—or, more usually, broke up altogether as seen in Fig. 8, and swam freely over the field as in Fig. 9. The only difference between them and the form which had yielded them was that they were smaller; but this difference in size rapidly disappeared.

These were the prevailing phenomena—at least those that were most readily seen; and this process might be the only one observed for days or even weeks. Indeed, we should have accepted this as a complete cycle, but for our former experiences.

At length by careful and constant scrutiny we perceived scattered among the rest forms of the same monad somewhat larger and plumper than the rest, and with a singular granular aspect towards the flagellate end. One of these is drawn at Fig. 10. These became more numerous, and we observed that they fastened themselves upon the ordinary forms, much in the same manner as was described in our last communication. Fig. 11 illustrates this some fifteen minutes after attachment; both forms freely moving their flagellum, and the pair swimming briskly over the field. There was a palpable absorption of the lesser by the greater, but the length of time this occupied was extremely various. When it was complete, there was no immediate change in the monad; it swam freely as before, but at length became more sluggish, and in the course of from two to six hours settled as a slightly flattened spherical body, the flagellum of which was seen slowly moving for a short time, but afterwards disappeared.

The length of time which these bodies remained in this condition was extremely uncertain; we have known them so remain, and, as the sequel proved, retain vitality for over thirty-six hours.

But during the whole time occupied in this resting condition, there was *no change* of any kind that could be detected by the highest powers we could employ. We have drawn a form in this condition at Fig. 12. Now we were several times perplexed by the sudden disappearance of these "disks" from positions in the field accurately marked, so that we might constantly and easily return to them. Was it that they resumed activity and swam away? We could only reply by watching them to the end. This was done, and Fig. 13, Pl. LII., gives a feeble idea of the result. The disk or sphere opened slowly, and there was poured out in continually increasing volume a glairy-looking fluid. It was not difficult to distinguish it from the fluid into which it was poured; the optical effect was like that produced by the pouring of strong spirit into water. But we could discover no *granules* in it. We exhausted every means at our disposal, and employed the highest magnification, but without result. Certainly nothing like the sporules we had seen before could be detected here. But we determined, after seeing this the second time, to make the field in which the outflow occurred the subject of special and continuous research. It was a comparatively clear field, and somewhat within the ring which most of these monads make around the covering glass when retained alive for any length of time.* We employed powers of from 2500 to 5000 diameters, and Fig. 14, Pl. LI., upper portion, represents a portion of the field the seventh hour after emission, but the tiny dots drawn there are at fault in presenting them as *opaque* objects, whereas they were semi-transparent, and of a yellowish hue. They were thus distinguished from the sporules we had seen at their emission, for they were dark and opaque. They came into view suddenly. A place where we could by no means employed see one, would all at once reveal one. After this their growth was comparatively rapid. Fig. 15 represents a portion of the same field as Fig. 14, drawn an hour and ten minutes after that in Fig. 14. Fig. 16 depicts another portion of the field drawn at the expiration of two hours more. From this time the growth was very rapid. The sharp-pointed bodies in Fig. 16 began to assume a rounder form, and the pointed end always became the end from which the flagellum developed. Fig. 17 was drawn at the expiration of another ninety minutes, when motion first showed itself; this, however, was not the motion usual to the monad, but a motion of horizontal vibration from *a* through *b* and *c* to *d* in Fig. 17, and then back again; after which it swam away, and rapidly became plump as in Fig. 18, and then was followed into the stages drawn from Figs. 2 to 9, Pl. LII., thus completing the cycle of its developmental history.

We have strong *negative* evidence that the larger forms which unite with the common ones (Fig. 10, Pl. LII.) are *not* subject

* 'Micros. Journal,' January, 1874, p. 8.

to the multiple fission which characterizes the great majority. They appear to develope from the invisible sporule, and when full grown to unite with a common form.

The history thus worked out is comparatively simple. The ordinary egg-shaped monad passes through a series of mutations of form until it settles as a minute sphere. A white cruciform mark suddenly appears, and is succeeded by others at angles to the first. A rapid interior action ensues, and at length the whole body of sarcode is divided into a large number of long bodies packed closely together, which separate as flagellate monads. Besides these there is a much smaller number of larger, rounder monads, of the same form, distinguished by their granular aspect. These seize and absorb the common form. The result is a still condition in the form of a sphere. This eventually opens and a fluid is poured out, or what appears like it; no sporules can be seen. The result of this, however, is the growth of minute specks, which we can only suppose came from invisible germs; and from these, forms grow like the parents, and the cycle is by them re-entered.

(*Experiments on Temperature, &c., to follow in the next issue.*)

## III.—*On a Simple Method of Preparing Lecture-Illustrations of Microscopic Objects.* By Rev. W. H. Dallinger, F.R.M.S.

(*Read before the* Royal Microscopical Society, *Jan.* 7, 1874.)

Most working microscopists have felt the necessity, in reading papers on their work, of accurate illustration. Mere enlarged drawings fail in matters of detail, unless extravagant labour is expended, and considerable skill employed. Even then, the light of an ordinary lecture-hall is not enough to enable the most distant of the audience to clearly see them. It is only by means of the lime-light and transparencies that really useful illustrations can be given. But, here, the difficulty is to prepare them accurately and inexpensively. Photography cannot be employed in all cases; and even where it can be, it involves more labour than most working microscopists can afford for every paper they may read. Drawing and painting on glass in the usual method is an art that it takes years thoroughly to learn; and to employ one who has learned it to draw from nature a highly magnified object, would be to introduce unnumbered errors of interpretation, unless our artist be a microscopist himself.

I obviate all these difficulties by the following method:—

On finely-ground glass, drawing with a blacklead pencil is as easy as drawing on London board. I get 4-inch squares of glass to suit my lantern, carefully ground on one side like the focussing glass of a camera. Now with the ground side up the camera lucida may be used with this as well as with drawing board if a piece of white paper be placed beneath it, and the object drawn in the usual way. For outlining and delicate shading I employ H H H H and H H H pencils; for deep shadows I use H B. By a very delicate employment of the pencil, shadows softer than can be secured by lithography may be made. The camera lucida, of course, is not necessary; we may draw with the eye and hand alone. If it be necessary to put in colour, it may be done, cleanly and carefully, *over* the shading; thus, one layer of colour suffices. Now, of course, although we have a perfect drawing of the object, with all the detail accurately given, *it is not a transparency.* But we can easily make it one. Thin some good pale *Canada balsam* with *benzine* to about the consistency of cream; and simply *float* it over the ground surface of your glass; pour off till the drop comes very sluggishly, then reverse the glass so that the corner from which the balsam was flowing off be placed upward. Let the return flow reach about the middle; then reverse it again, and move it in several directions to get the balsam level. This may be done with a very little practice so that the surface shall be undistinguishable from glass. We have now a *perfect transparency.* All that is required

is twenty-four hours for hardening (keeping the glass level), and then another square of glass fastened on to it by strips of paper at the edges, with small pieces of card at the corners to prevent contact, and it makes an admirable lantern transparency.

In the same way the drawings of other observers may be copied; by simply laying the ground glass *upon* them, the figures are seen through, and may be well and accurately drawn. In this way I have prepared some of Dr. Beales' most complicated drawings for the screen, and enlarged them to 12 or 14 feet, preserving every detail; while a little crimson, lake, and prussian blue, used with a sable pencil, give all the effects of staining.

I forward two specimen transparencies chosen promiscuously from a large number of drawings from life. [These were exhibited to the meeting.—Ed. 'M. M. J.']

## IV.—*A Method of Dissecting Podura Scales.*

By F. H. Wenham, V.P.R.M.S.

At the last scientific meeting of the Royal Microscopical Society, of Dec. 10th, a Podura scale was exhibited by me, of which I have formerly made mention. This was a strongly-marked specimen of Richard Beck's, presented to me by Dr. Gray. In mounting, the cover had rotated, drawing a portion of the ribs with it. One of the note markings had been carried round half way, like turning the handle of a copying press. The various positions of the twisted ribs were shown with such remarkable distinctness, that no question could be raised of their reality.

Unfortunately this curious object got smashed during the evening. Both the pair of thin glass disks were starred; the centre of fracture started just on the scale I wished to preserve, so that it could not be seen. I separated the disks, which contained many large specimens, and with a keen-edged scalpel attempted to remove some of them, but found that they adhered to the glass with great tenacity, as if they were glued thereto. There was no alternative but to pass the edge of the blade on the glass at random, and collect the scrapings, which were at once remounted. In these I am rewarded with a fine collection of fragments, some puckered up in ridges, giving a perspective of the ribs—pieces are torn off, and others cut clean through. Fig. 1 is an outline sketch 1800 diameters at the

side of half a scale cross-cut through obliquely. Fig. 2 is another portion from the middle: both were drawn under a $\frac{1}{75}$th.* The cut ends of the ribs stand out *over* the membrane as plainly as the teeth of a rake, thus giving a curious confirmation of Mr. Joseph Beck's statement, that the projection of the ribs is on the under

* The outlines in the cuts were traced with the camera lucida. My intention was to send a photograph, but as the mirror of my solar reflector swings from below instead of in the axis of the instrument, and the shutter of the dark room faces east and west, I found that I could not get the light central enough early in January with the sun on the meridian. I will avail myself of the earliest opportunity of producing these photographs.

side, or that next the body of the insect. In mounting the scales the cover is laid on the back of the Podura, and those adhering to the glass are drawn off, consequently the outside is next the cover. Now in my specimen the keen knife-edge has first met the raised ribs, and then undercut the membrane farther back. Had this been severed first it must have covered the rib ends, which would not project, but have lain backwards beneath. I shall be happy to show this specimen to anyone interested in the question.

None of the minute detached sections show any beads, for in the Podura these are illusory. In the entire scale they may be made to appear throughout more or less plainly by conjuring the illumination, particularly if the object-glass is set a little out of proper adjustment. I have on several occasions stated, that a determination of structure in finely-marked transparent entire objects cannot be entirely relied upon for accuracy, as the refraction and interference of one portion influences the adjoining parts. I have an instance of this. Some gum-arabic has run under part of a Podura scale. In the dry portion beyond this for the remaining distance of the scale, the interference bands from the edge of the gum, cause the whole of the ribs to appear as lines of beads, which follow the exact curved outline given by the gum.

These spurious beads, with a touch of the illumination, appear with a plainness that might delight the eyes of the few that would believe in their reality.

My accident has shown how clean detached fragments of all sizes and sections may be obtained. Dr. Pigott, in order to sustain his theory, has selected that well-known impracticable object, a mashed Podura scale, which has been jumbled into a bead-like mass or rubbed between two surfaces, reminding me of a lettuce-leaf that has been trodden upon. I consider this more like structure destroyed than developed. In this question, believing as I have done, and in what others have shown before me, I have no pet counter-theory of my own to support. Some respect must be paid to reasoning by analogy from various scales, and the gradual transition of insect hairs into scales. It must be remembered how systematically Mr. McIntire has gone into the question. Also, in the last Journal, we have a paper by Mr. G. W. Morehouse, "On the Structure of the Lepisma Saccharina Scales." The investigation has been made calmly and carefully, with the highest powers practically useful, by one who well understands his work. He arrives at the conclusion that both the small and big beads (stated to constitute this object also) are *spurious* for a similar reason.

## NEW BOOKS, WITH SHORT NOTICES.

Experimental Researches on the Causes and Nature of Catarrhus Æstivus (Hay-fever or Hay-asthma). By Charles H. Blackley, M.R.C.S. London: Bailliere, Tyndall, and Cox, 1873.—It is somewhat singular that much as has been done in the direction of discovering what is the exact nature of the cause of hay-fever, it is yet a question which can hardly be considered perfectly answered. Yet certainly we cannot but say that the author of the present work has done much toward its discovery, and has certainly, in our opinion, put us better on the road than we were previously. Of course our readers are aware that already much had been done toward the completion of the theory of the pollen origin of the disease when Mr. Blackley took the question in hand. But he has certainly merited great credit for the ingenuity he has displayed in inventing and constructing the numerous contrivances which are described in this volume, for the purpose of collecting, under certain given conditions, the amount of pollen present in the atmosphere. Indeed in all that relates to the microscopical examination of the matter he has been at considerable pains to render his conclusion irrefutable, and hence his opinions deserve most careful consideration. But we think it a pity that he has in many cases gone over ground which really was sufficiently trampled already, that he has in dealing with the question endeavoured to prove facts which might be regarded as already adequately cleared up. Of course one of the great difficulties of his view of the causes of this disease is the fact that several of the symptoms presented by the more severe cases of the malady are not referable to the influence of the pollen grains as externally irritating the mucous membrane. But the author offers the following remarks which seem to us worthy of consideration:—"I have found by experiment that the granular matter of pollen may, by dialysis, be made to pass through membranes which are thicker than those that line the air vessels and bronchial tubes; and from this circumstance I think that it is highly probable that the finer particles of this matter do, in some cases, pass through the mucous membrane of the respiratory passages, and by getting into the circulation in this way give rise to the constitutional symptoms we see developed in some cases." But Mr. Blackley here has the difficulty that it is only in some cases, not in all, and that it remains to be proved by him, as it is not an universal fact. Again, there is the circumstance that this disease is not at all general or common, a fact which we ought to expect if it was due to so universal a cause as he and many others allege. His experiments relative to the quantity of pollen that is taken in were most satisfactorily conducted. But they lead us to nothing. In fact we are more than ever disposed to fall back on Dr. Bostock's view, that hay-fever, as it is termed, is not due to the pollen from grasses, but is simply the result of heat upon some peculiar constitutions. Still, the author deserves every consideration from the very extended nature of his investiga-

tions, and it is not impossible that both heat and pollen may have to do with the development of hay-fever. He says in the conclusion of his fourth chapter, p. 153, that "I have shown that pollen of all kinds will give rise to some of the symptoms of hay-fever, and that all the other so called causes have little or nothing to do with generating the disease. I have also shown that the actual attacks of the disorder as they occur in the summer, are caused by the pollen which floats in the atmosphere at this time." Then he goes on to say that in the atmosphere at a certain height there is a zone which contains a greater amount of germs and spores than are found at any other height, and he inquires how far this has to do with the spread of disease. But such questions of course cannot be answered definitely. Indeed, it seems likely that such a distinct stratum in the atmosphere has no existence whatever. It seems to us that the author has failed in one portion of his study. He has done all parts of the microscopic work creditably, as far as regards the inquiry as to the amount of pollen in the air; but he has not sufficiently examined the matter extracted from the lungs and eyes of sufferers from this disease, to observe beyond question that pollen grains were present in all cases. Still, it must not be supposed that we think his views undeserving of consideration, for they are well matured and elaborately laid down; on the contrary, we think most highly of them, and we have pleasure in commending his book to our readers' attention.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*Milne Edwards on the Circulation in Limulus.*—According to late investigations which he has made on this subject, it would seem that the circulating apparatus of Limulus is more perfect and complicated than that of any other articulate animal. The venous blood, instead of being diffused through interorganic lacunæ, as in the crustacea, is, for a considerable portion of its course, enclosed in proper vessels with walls perfectly distinct from the adjacent organs, originating frequently by ramifications of remarkable delicacy, and opening into reservoirs which are for the most part well circumscribed. The nutritive liquid passes from these reservoirs into the branchiæ, and, after having traversed these respiratory organs, arrives by a system of branchio-cardiac canals, in a pericardiac chamber, then penetrates into the heart, of which the dimensions are very considerable. It is then driven into tubular arteries with resistant walls, the arrangement of which is exceedingly complex, with frequent anastomoses, and of which the terminal ramifications are of marvellous tenuity and abundance.

*The Brachiopod in Embryo* is the subject of a paper read before the late meeting of the American Association by Professor Morse. We may say of it that the embryo commences life as a little worm of four segments, and after enjoying itself in swimming freely in the water for awhile, attaches itself to the sea bottom by its posterior segment,

and settles permanently. The middle segment then protrudes on each side of the head segment, and gradually encloses it, thus producing the dorsal and ventral shell so characteristic of the entire class. This unlooked for, simple development could not have been predicated by any study of the adult animal, but remarkably sustains the homologies insisted upon two years ago by Professor Morse in his papers upon the subject.

*A supposed New Potato Disease.*—A paragraph has appeared in several scientific papers, quoted from the 'Zeitschrift für Parasitenkunde,' stating that Professor Hallier of Jena has described a new potato disease, which made its appearance last autumn in the neighbourhood of that town, the disease being indicated by the presence of a purple web, and the appearance of a number of black spots on the skin, referable apparently to the perithecia of a pyrenomycetous fungus. We learn, says 'Nature,' from the Rev. M. J. Berkeley, that this so-called new disease is nothing but the well-known "copper-web," which is in some years very destructive to asparagus, mint, and other crops, and has been known in some instances to attack the potato. The description in the 'Zeitschrift' is identical with this familiar parasite. Figures will be found in Tulasne's 'Fungi Hypogæi,' under Rhizoctonia, showing that the so-called perithecia are spurious. Mr. Broome has detected the form of fructification known as Conidia.

## NOTES AND MEMORANDA.

**An Immersion Tube for the Microscope.**—The arrangement that was exhibited at the last meeting, by Mr. E. Richards, F.R.M.S., consists of a tube having the universal screw at each end, over which the immersion tube is fitted with a cap, after the principle of the protecting cap which the inventor brought before the notice of the Society in June, 1872. It will admit of immersing the tube deep enough to bring objects into view in about eight or ten inches of water; the powers most useful are from four inches to one inch. The mode of using this accessory is merely to screw one end of the inner tube to the microscope body, and screw the object-glass into the other end, after which put on the immersion tube, the position of which must be regulated according to the power intending to be used. It can be used with any microscope which has an aperture in the stage large enough to admit the tube passing through, or where the stage can be removed.

**Professor Betz's mode of preparing Sections of the Brain.**—This is given very fully in Dr. Batty Tuke's 'Journal of Mental Science.' It is stated that the Professor has lately produced brain-sections which have attracted very considerable attention in Vienna. His specimens are of vast extent. He appears to be able to produce thin sections of an entire hemisphere. Dr. Tuke gives his method of hardening and cutting as it is stated in the 'Correspondez Blatt

der deutschen Gesellschaft für Psychiatrie und gerichtlich Psychologie.' The method of hardening is as follows,—observing that differences exist in the treatment of the spinal cord, cerebrum, and cerebellum. The spinal cord, after the careful removal of the dura mater, is placed in spirit of from seventy-five to eighty per cent., which is tinged a clear brown colour by the addition of iodine. After from one to three days, during which the preparation must stand in a cool temperature, the pia mater and the arachnoid are also removed, the specimen remaining in the spirit, to which a few drops of iodine must be added daily for three days, maintaining an ordinary temperature. It is then transferred to a three per cent. solution of chromate of potass, and back again to the cool temperature. Here it hardens thoroughly, which is known by the fluid becoming turbid, and by the formation of a brown deposit upon the preparation. When this occurs it must be immediately thoroughly washed with water, and immersed in a solution of chromate of potass, from half to one per cent. strength, in which it will not become too hard or brittle. Preparations of cerebellum can only be made when it has been taken from a perfectly fresh body. Before immersing it in the iodine spirit, the vessels and membranes must be carefully removed, especially at the vermiform process and the "square lobes"; and cotton wool should be stuffed into the sulci on either side of the process, the rhomboidal groove, and the nates and testes, should they be in the specimen, so as to render the passage of the fluid into the deeper parts more easy. The preparations should rest on cotton wool. The iodine spirit should be quickly increased in strength. After from seven to fourteen days the specimen should be placed, provided it does not give to the finger, in a five per cent. solution of chromate of potass. The *great brain*, after being divided in half through the length of the corpus callosum, is laid in weak iodine spirit. After some hours the separation of the membranes in the fissure of Sylvius, and at the tail of the corpus callosum, should be commenced, so as to allow of the permeation of the spirit. The preparation must stand in a cool place (during summer in an ice-cellar). After from ten to fourteen days it is removed to a four per cent. solution of chromate of potass. When sections are to be taken, it must be washed carefully in water. Betz endeavours to avoid all rubbing of the knife on the surface of the preparation, and sticking of the section on the upper surface of the blade. To this end he has had constructed a knife whose upper surface is convex, the under one concave, the radius of the lower one being somewhat smaller than that of the upper. The blade is from one and a half to twice as long as it is broad, the thickness being one-third of the breadth. For large cross-sections, as for instance through the whole hemisphere, Betz uses a knife whose blade is twenty-one centimètres (eight and a quarter inches) long by ten centimètres (four inches) broad. This form of knife (hatchet?) makes it possible to keep the surface of the preparation and the section constantly wet by means of dropping spirit, so that rubbing on the one and sticking of the other may be avoided.

**Stereoscopic Pictures of Objects seen with the Binocular.**—Dr. J. G. Richardson stated at a recent meeting of the Academy of Natural

Sciences of Philadelphia, that by taking a photograph through each tube of the binocular microscope and afterwards combining these two pictures by looking at them in the ordinary hand stereoscope, we should secure a perfect and complete reproduction of the exquisite image in relief obtained by the aid of double-tubed instruments, and he recommended that the method should be tried at an early opportunity.

## CORRESPONDENCE.

### On Mr. Wenham's Reply to Dr. Woodward.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, Mass., U.S.A., *December* 16, 1873.

Mr. Editor,—I have no intention of engaging in the "battle of the glasses," but as I have been conversant from the commencement of the "battle"—more so probably than Dr. Woodward—with some of the facts, I wish to offer in your pages a few remarks on Mr. Wenham's "Reply to Col. Dr. Woodward," printed in your December issue.

Mr. Wenham writes, "The controversy has been so long and tedious, that it is not a matter of surprise that they (*i. e.* the points of the controversy) should be forgotten." I must thank Mr. Wenham for the suggestion. Let us inquire whose memory has failed. That the controversy has been so long is in a measure owing to the necessities of the printers, and the interposition of the Atlantic Ocean between the combatants. From the time that Mr. Wenham *writes* his *optical laws*, it requires three—perhaps four months before a reply can be published in London. It may have been tedious to Mr. Wenham to reiterate the impossibility of doing what had often been done, but I can assure him that his efforts in the cause have been eagerly looked for by microscopists at this side.

It is certainly "a matter of surprise" that Mr. Wenham himself is the one that has forgotten the subject of the controversy. "I cannot think that I have anywhere stated distinctly that it was not possible to construct an object-glass with an immersed angle exceeding 82°" (December number, p. 256). In No. XXVII., p. 118, May, 1871, he writes, "and whether the object is mounted in balsam or not —I challenge Dr. Pigott, OR ANYONE ELSE [capitals mine], to get, through the object-glass with the immersion front, a greater angle, or any portion of the extreme rays that would in the other case be totally reflected, as NO OBJECT-GLASS CAN [capitals mine again] collect image-forming rays beyond this limit."

That is, and has been, the question, and the whole question in controversy. There is no allusion whatever to "reduced angle." There is no intimation that Mr. Wenham had ever constructed, or dreamt (which is about all that he now claims) of constructing, a lens capable of such a performance, but—if words have meaning,—a distinct unqua-

lified assertion that it cannot be done,—though Mr. Wenham's memory is too poor to recall what he had written,—and a peremptory challenge to all mankind, "to get, through an immersion object-glass," any more than he would get through any other. When Mr. Tolles wrote that it could be done, and had been done repeatedly, Mr. Wenham says,* "The ground is safe, and anyone that ignores or decries such a definite principle must expect to forfeit all respect for his optical knowledge." Perhaps Mr. Wenham has forgotten writing that also.

Mr. Wenham, referring to the object-glass sent to him last year, says that "The object-glass was not professedly an immersion one." This is astonishing. I will say that it is professedly immersion, and nothing else, will *not* perform at all well dry, and it is most surprising that Mr. Wenham could have thought that a dry lens should have been sent to him to illustrate a question that related solely to immersion lenses. No wonder that he found his dry lens, made twenty years ago, the better glass.

That Mr. Wenham was not positively informed that the glass was immersion only, is easily explained. It was expected that Professor Markoe, who knew all about it, would deliver it personally to Dr. Lawson or to Mr. Wenham, but, unfortunately, both those gentlemen were absent at the time Professor M. was in London.

Mr. Wenham writes of this glass, "I should have been quite content to try it if the adjusting collar had been pinned fast by the sender. . . ." This is quite a valuable suggestion for the future when a glass is to be sent to a novice; but, in sending to one of the most renowned microscopists in Europe, it was not supposed needful to give him instructions in manipulation, as to how to find the maximum angles, as it would have been if sent to a tyro who had yet to learn the A B C of his instrument; and at that time there was no knowledge or suspicion here that Mr. Wenham had other interest or connection with any one firm of opticians than he had with any other.

Finally, Mr. Wenham again says, "I am at length told that the object-glass defines best with a cover $\frac{1}{70}$th thick." I must ask Mr. Wenham to say who told him so? Mr. W. has already been requested to give his authority for his statement of Mr. Tolles' "admission" about this same lens, but he has forgotten to reply. I hope that he will not find it convenient to forget to answer my question.

I will yield precedence to no one in my appreciation of Mr. Wenham's services for many years past, by his beautiful inventions and contrivances for improving the microscope; all microscopists are under obligations to him for his valuable additions to their instruments. Now they have greater cause than ever to thank him, for his recent papers have drawn the attention of microscopists throughout Europe and America to the work of a brother optician more effectually than anything else could have done, and have exhibited more conclusively the difficulties overcome, and illustrated more strongly the skill manifested in so overcoming them, than anything that the other would have ventured to say for himself.

CHARLES STODDER.

* 'Monthly Microscopical Journal,' vol. viii., p. 293.

## A Substitute for the Tint-glass Reflector.

*To the Editor of the 'Monthly Microscopical Journal.'*

Upper Holloway, *December* 18, 1873.

Sir,—I wish to call the attention of those interested in microscopical work to a modification of the existing apparatus used for microscopical drawing. The instrument I use and which I find more serviceable than any other for this purpose, on account of its simplicity, consists of the thinnest possible covering glass placed at a proper angle in front of the eye-piece of the microscope. The advantage of this thin film of glass over the camera lucida, the neutral tint reflector, or the steel disk, is that it enables the pencil to be easily followed in tracing the image which is thrown upon the paper below. An ordinary piece of white glass does not answer the purpose, as it throws two pictures of the object. This doubling of the image is reduced to nothing in proportion to the thinness of the glass. The instrument which I have had made for me by Mr. Sutton, instrument maker, 108, Holloway Road, is sold at a very moderate price. It is composed of a brass collar to affix to the eye-piece of the microscope; this carries two light brass arms, between which, mounted in a brass ring, revolves the glass, so that it may be placed at the required angle.

I remain, Sir, yours, &c.,

W. Kesteven, Jun.

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## An Error in Mr. Plumer's last Letter.

*To the Editor of the 'Monthly Microscopical Journal.'*

*January* 12, 1874.

Sir,—Will you allow me to correct a misprint in my letter in the January number of your Journal? At the beginning of the line towards the end of the first paragraph, instead of 1½th read ½th.

Believe me, yours faithfully and obliged,

J. J. Plumer.

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## Prototaxites.

*To the Editor of the 'Monthly Microscopical Journal.'*

McGill College, Montreal, *Jan.* 1, 1874.

Sir,—Though I do not propose to continue the controversy which Mr. Carruthers has raised respecting Prototaxites, I must ask permission to direct the attention of your readers to three points in his rejoinder in your November number.

First, he abandons a great part of the essential conditions of the case in the statement that "the mode of occurrence of the fossil has nothing whatever to do with the matter." This statement neither I nor any other palæontologist can admit. In the study of any doubtful fossil, geological age and conditions of occurrence, fossil associates and state of preservation furnish most valuable guidance; and neglect of these aids has been a fruitful source of error.

Secondly, in the one point to which he now narrows his argument,

that of microscopic structure, he contents himself with assertion, and with citing witnesses who have had no communication respecting the matter except with himself. In answer I can only repeat the statement of my confident belief in the accuracy of my own observations. Vegetable histology has been with me an almost life-long study, more especially in its application to the structures of fossil plants; and long practice in examining them in all states of preservation and prepared by various methods, has given me too much confidence in my own results to allow me to defer to the opinions of observers of less experience in this special department. More especially must this be the case when their own statements render it plain, as I pointed out in my former communication, that they have not had distinctly before their minds the gradations of palæozoic structures, and of disintegration of woody tissues which serve to bridge over the space between modern gymnosperms and such organisms as Prototaxites. The materials to illustrate this exist in my own cabinet; but it would require a long memoir and many engravings adequately to inform those who have not gone over the series of recent and fossil woods that led me to understand Prototaxites, and thereby to enlarge our views as to the range of structure of the more ancient land plants, and as I hope to pave the way for the discovery of yet more strange and elementary forms in the Silurian flora yet to be discovered. Time and means to give such illustrations are both wanting at present, and new facts and specimens are urgently calling for investigation.

Lastly, as Mr. Carruthers volunteers to exhibit and explain the specimens which (I fear too incautiously) I presented to the British Museum, allow me to say that I would prefer to take this duty on myself, and to furnish to anyone who may take the trouble to study this remarkable plant, typical specimens, and also some material for comparison.

Yours truly,

J. W. Dawson.

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## Triceratium fimbriatum, Wall.

*To the Editor of the 'Monthly Microscopical Journal.'*

Forest Rise, Leytonstone, *January* 3, 1874.

Sir,—Having just now opened the September number of your Journal, I find at page 137 some remarks on this species by Dr. Arthur Mead Edwards, a well-known diatomist, occasioned by a paper by Dr. Woodward in the 'Lens' for April, 1872. I have not seen this number of the 'Lens,' nor consequently the figures in Dr. Woodward's plate on which his remarks are founded. I am surprised, however, to find that Dr. Edwards comes to the conclusion that this species should be rejected, and united with *Tric. favus*, Ehr., and I cannot but think he has formed his opinion on insufficient data. I grant him that Ralfs has made *Tric. fimbriatum* a synonym of *Tric. favus* ('Prit. Inf.,' 1861, p. 855), and so has Rabenhorst ('Eur. Flor. Alg.,' 1864, p. 315), but notwithstanding these authorities and my strong inclination to unite rather than to multiply species, I still venture the opinion that the species are distinct, and that the difference in the valves can be at

once recognized by anyone conversant with the two forms. Dr. Edwards seems to base his opinion on his examination of Möller's Typen Platte and Dr. Woodward's figures; of the latter I can say nothing as I have not seen them, but as to the former I would observe that in my Möller's Typen Platte, the specimen of this diatom is a three and not a four sided form, and corresponds exactly with Dr. Wallich's figure and description, and is identical with my other specimens of *Tric. fimbriatum*. Möller is undoubtedly in error in assigning the species to Brightwell instead of to Wallich, but this does not affect the question whether or not it is a distinct species, and whatever Möller's inaccuracies in his index I feel sure everyone must admit the marvellous skill which has produced such a specimen of mounting as his Typen Platte. *Tric. fimbriatum* can, I contend, be easily known by the convexity of its sides. Of *Tric. favus*, the original type of the genus, I have examined hundreds of specimens, but never found one of similar outline to *Tric. fimbriatum*. The sides of *Tric. favus* are "planis aut leviter convexis" ('Kutz. Bacill.,' p. 139), or as Ralfs has it, "straight or slightly convex" ('Prit. Inf.,' l. c.). Those of *Tric. fimbriatum* are, however, convex, forming, as Dr. Wallich, while noting the constant character of the outline as a remarkable feature ('Micr. Journ.,' vi., p. 248), goes on to describe, the arcs of circles, the radius of which is the distance to the opposite angle. Though neither have I seen Dr. Wallich's original specimens of *Tric. fimbriatum*, I have in my cabinet valves of it from Saint Helena, the habitat mentioned by him, which entirely bear out the above, and would alone, I think, cause Dr. Edwards to modify his opinion. I would observe that Dr. Wallich's figure ('Micr. Journ.,' vol. vi., Pl. 12, *f.* 5) is a little inaccurate in showing the rows of hexagonal cellules as straight, instead of following the same curve as the sides, a feature which adds much to the beauty of the form.

A similarity in outline to *Tric. fimbriatum* as to the convexity of the sides which exists in the figure of *Tric. grande* (Brightw., 'Micr. Journ.,' vol. i., Pl. 4, *f.* 8), which apparently ('Micr. Journ.,' vol. iv., p. 276) is the same as *Tric. orientale* of Harv. and Bail., leads me to notice the double description by Dr. Greville of *Tric. Robertsianum*. In 'Micr. Journ.,' vol. xi., Pl. 9, *f.* 9, he describes and figures a diatom from Sydney under this name, and, apparently forgetful of this description, again in 'Micr. Trans.,' vol. xiv., p. 7, Pl. 2, *f.* 22, where he queries it as identical with *Tric. grande*. Though Dr. Greville's two descriptions and figures of *Tric. Robertsianum* differ somewhat, yet they relate, I conclude, to the same form.

Whether they and *Tric. grande*, *Tric. orientale*, and *Tric. fimbriatum* should all be united in one species, I have no means of judging, beyond comparing the descriptions, figures, and habitats above referred to; but I think it quite probable an examination of the original specimens might lead to that conclusion. However that might be, I do not see my way at present to ranking *Tric. fimbriatum* under *Tric. favus*.

Yours obediently,

H. Ramsden.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *January* 7, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read by the Secretary, and the thanks of the meeting were voted to the donors.

The Secretary having called attention to the circumstance that their next meeting would be the anniversary, read the following "house list" of gentlemen recommended by the Council for election as officers of the Society on that occasion:—

As *President*—Charles Brooke, Esq.‡

As *Vice-Presidents*—*Dr. Braithwaite, *Dr. Millar, Mr. W. K. Parker, and Mr. Wenham.

As *Treasurer*—Mr. Stephenson.

As *Hon. Secretaries*—Mr. H. J. Slack and Mr. Charles Stewart.

As *Members of Council*—Mr. Bell, *Mr. F. Crisp, Dr. W. J. Gray, *Mr. Ingpen, Mr. McIntire, *Mr. Henry Lee, *Mr. Loy, Dr. Lawson, Mr. Perigal, Mr. Sanders, Mr. Tyler, Mr. T. C. White.

The Secretary having read the bye-law relating to the proposal of other candidates, invited any gentlemen present who wished to do so to fill up and sign in due form such nominations, and intimated that the names of persons so proposed would be printed with the others on the ballot papers. He also stated that, in accordance with a suggestion made by Mr. Beck at the last annual meeting, the lists of gentlemen who were proposed would be forwarded to the Fellows previously to the annual meeting.

The Secretary asked that two gentlemen might be appointed Auditors of the Society's accounts, and read the bye-law which provided for their election. Mr. Suffolk was then proposed by Mr. Garnham, and seconded by Mr. Ingpen; Mr. E. W. Jones was then proposed by Mr. Curties, and seconded by Mr. Ingpen.

The President having put the question to the meeting, declared Mr. Suffolk and Mr. Jones to have been unanimously elected Auditors of the Society.

The Secretary said he had received a letter from Lord Osborne upon the subject of the drying of rotifers, in which he stated that he had recovered the species mentioned by Mr. Davis, and offered to supply anyone with specimens who would send him a small tank for the purpose. His lordship had changed his address—he now resided at Sidmouth.

The Secretary read a short communication from the Rev. W. H. Dallinger, descriptive of a simple method of preparing lecture-illustrations of scientific subjects, a specimen of which was placed upon the table. The drawings were made with a pencil upon finely-ground

‡ Those with an asterisk before their names are proposed as new members.

glass, and were then covered with balsam so as to increase the transparency and enable them to be shown by a lantern.

The Secretary said they had received a further communication from Mr. Dallinger, in which he described the effect of temperature upon the very minute organisms which he had previously written about. The paper was of much interest, and would be published in the Journal, but, owing to the pressure of other matter, it would be taken as read.

Mr. Charles Stewart, Secretary, said that they had also received a highly interesting paper "On the Origin and Development of the Red Blood Corpuscles in the Human Ovum," by Dr. H. D. Schmidt, of New Orleans. The paper was illustrated by some of the most beautifully-executed pencil drawings he had ever seen, which would be printed in the Journal, where they would have the opportunity of reading the paper *in extenso*. It would not be possible to read the whole of it to the meeting, in consequence of there being other matters to occupy their time, but he would endeavour to give them an outline of its contents, and just roughly draw some of the illustrations on the board, and then he should refer them to the Journal for the more minute details.

Dr. Matthews inquired what was the presumed age of the ovum?

Mr. Charles Stewart said that the exact age was not named, but it was stated that the woman had not menstruated for two or three months, but the signs of pregnancy had only been for three weeks; the size of the ovum was given as 2½ centimètres.

Dr. Matthews said he had asked the question because he had with him an ovum the exact age of which he knew positively. The patient from whom it came had gone to the end of an ordinary pregnancy, and was delivered of a child, which died. The usual consequences of labour were passed through, and lasted the ordinary period; at the end of the month she menstruated, and at the end of another month she aborted, and the ovum which he produced was the result, so that he believed there could be no doubt as to its being a month old. He could positively say that there never was any appearance of blood at all either in the placenta or in the vesicle.

Mr. Stewart said that in the case described by Dr. Schmidt the blood was contained in the walls of the vesicle.

The President thought that the specimen exhibited by Dr. Matthews must be a much more developed ovum, the size of the embryo being nearly half an inch.

Dr. Matthews said he had mentioned the specimen shown by him because there certainly never was any more appearance of blood in it than it showed at the present time, and he obtained it within three hours of the time when it was extruded.

The President thought it quite likely that the circumstances referred to in the paper were the result of abnormal development, and that as this went on some red blood corpuscles might have been produced. It was quite possible that where one condition was abnormal all the rest might be so too; so that, supposing it to be a case of abnormal development, a small sac of blood disks might have been

developed, just as in other instances of abnormal growths a piece of skull with hair, or part of a jaw with teeth, might be produced.

Mr. Charles Stewart stated, with respect to the one shown by Dr. Matthews, that it was evidently injured; it had escaped from the envelopes, and there might consequently have been a loss of fluids, whereas the one described in the paper was discharged entire. It was probable that it had been dead some time, and that there was therefore a stagnation of the blood; and that, even in the case of a rupture, it would not be so likely to escape as readily as under other conditions.

Mr. Alfred Sanders read a paper, entitled "Notes on the Zoosperms of Crustacea and other Invertebrata." The paper was illustrated by drawings, and will appear in another number of the Journal.

The thanks of the meeting were unanimously voted to Mr. Sanders for his communication.

Mr. Slack called the attention of Fellows present to some fine slides of Aulacodiscus, which had been presented to the Society by Captain Perry, and also to a tank microscope brought for exhibition by Mr. Richards; it had a long shield tube over the object-glass, enabling it to be placed a considerable depth into the water.

Mr. Richards said the main object of his contrivance was to be able to bring the object-glass within a focussing distance of the bottom of the tank, where often many organisms were found which would be absolutely destroyed by any attempt to remove them. The tube upon this microscope would allow it to focus to a depth of 8 inches; it could be used with powers from $\frac{2}{3}$ to 4 inches, and with any microscope which would admit of the tube passing through the stage.

Mr. T. C. White thought that it would be an advantage if the stand were made so that it could be used for all portions of the tank.

Mr. Richards said there was no difficulty in doing that; his object was in this case only to show the use of the immersion tube.

Donations to the Library and Cabinet from December 7, 1873:—

| | From |
|---|---|
| Land and Water | *The Editor.* |
| Nature. Weekly | *Ditto.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| Proceedings of the Literary and Philosophical Society of Liverpool. No. 27 | *Ditto.* |
| Spectrum Analysis as applied to Microscopical Observations. By W. T. Suffolk, F.R.M.S. | *Author.* |
| Jornal de Sciencias Mathematicas Physicas e Naturaes. Three Parts | *Academy of Sciences at Lisboa.* |
| Journal of the Linnean Society. Nos. 73 and 74 | *Society.* |
| Canadian Journal. No. 79. | |
| Popular Science Review. No. 50 | *Editor.* |
| Three Slides of recent Diatoms | *Capt. J. Perry.* |

Alfred Carpenter, Esq., M.D., J.P., was elected a Fellow.

WALTER W. REEVES,
*Assist. Secretary.*

THE

# MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1874.

## I.—THE PRESIDENT'S ADDRESS.

*Delivered before the* ROYAL MICROSCOPICAL SOCIETY, *February 4th*, 1874.

GENTLEMEN,—It is not in the power of your President to follow the example of his talented predecessor in this chair in laying before you the valuable results of original and laborious researches in an important branch of natural history, in the pursuit of which the microscope is, at all events, a valuable adjunct; and this is the more to be regretted, when he remembers the very flattering manner in which his name was received at the preceding nomination.

In common with the rest of your officers and Council, a hope has for some time past been entertained, and not altogether groundless, that ere this you might have been welcomed in more permanently allotted premises amongst the other Royal Scientific Societies in Burlington House. A deputation from this Society waited on Mr. Layard when in office as First Commissioner of Works, &c., and was very favourably received by him. He appeared to consider that the services the Society might be able, when occasion requires, to render to the Government in the employment of the best instruments in existence by the most competent observers, and in the information that might be given, if required, to those in the public service on the use of the microscope, would be a not inadequate return to the public for accommodation in Burlington House; and this opinion he has since confirmed in private correspondence. So well were the views of Mr. Layard understood at the Office of Works, that some time last year the Government contractor called on the Assistant Secretary, and offered to furnish "our rooms in Burlington House" on the same terms as those on which he supplied the Government, the name of our Society having been given to him at the Office of Works as one of those to whom accommodation had been assigned. Mr. Layard, however, unfortunately relinquished his office without leaving any official record of his views, which were in consequence ignored by his successor, Mr. Ayrton, who expressed to a deputation from this Society his determination not to appropriate the rooms in question to any particular body, but to keep them vacant for any possible scientific inquiry the Government might require to conduct. Mr. Ayrton has, however, been suc-

ceeded in office as First Commissioner by Mr. Adam, who has kindly intimated his willingness to reconsider the question of the much-desired accommodation of this Society in juxtaposition with other kindred societies; and it is sincerely to be hoped that his successor in office may not be less favourable than himself to the aspirations of this Society respecting accommodation in Burlington House.

The principal features of the papers published in the Transactions of this Society during the past year have been noticed in the Report; but there are some to which special attention may perhaps with advantage be directed. Among them may be mentioned a paper "On the Structure and Function of the Rods of the Cochlea in Man and other Mammals," by Dr. Urban Pritchard; this paper did not seem to attract the amount of attention it deserved, affording as it does a clue to the mechanism by which one of the most important functions of the ear is fulfilled; it perhaps, therefore, may not be undesirable to make a few observations upon this subject. The internal ear consists of two important parts, namely, the semicircular canals, and the cochlea, cavities imbedded in the densest bone in the body. The semicircular canals consist of three tubes of a form nearly approaching to that which their name implies, and these are placed with respect to each other in a very remarkable position; in all classes of animals they lie in three planes perpendicular to each other, so that each is placed at right angles to the other two. They are found in all classes of animals, from the highest down to the cartilaginous fishes, and they occupy the same position in all. Their chief function is undoubtedly to determine the direction of sound: valid reasons have long since been assigned by the writer for assuming this to be the case.

The other important organ is the cochlea. This may be described as a conical tube spirally convoluted upon itself, resembling an ordinary snail-shell, and supposing it placed with the axis of the whorl vertical, it is divided horizontally into two portions by a thin plate, partly of bone and partly membrane, running through its whole length from the larger to the smaller end, the membranous portion consisting chiefly of a great number of transverse fibres. On these rest the outer ends of the "rods of the cochlea," or the "rods of Corti," the inner extremities of which rest on the bony portion of the spiral lamina. These rods consist of two portions jointed together, which form a kind of arch, much resembling the widely-extended finger and thumb. It is stated in the paper that the number of the inner and outer rods is not alike, three of the inner corresponding with two of the outer, thus . ˙ . ˙ . ; but it appears more probable that their arrangement is alternate throughout, thus . ˙ . ˙ . ˙ . ˙ . ˙ . ˙ . ˙ . ˙ ., and not thus . ˙ . ˙ . ˙ . ˙ . ˙ . ˙ . ˙ ., each of the outer rods being in fact articulated between two of the

inner ones. The numbers in the two series will, in that case, differ by one only, and not be in the ratio of 2 to 3, as the author supposes. The number of the rods is assumed by the author to be not less than 5000, a number far greater than the differences of pitch that the human ear can be required to appreciate. The cochlea is furnished with a very large bundle of nerves; so large indeed, as probably to supply a fibre in relation with each one of the rods. It is well known that the pitch of sounds depends on the length of the waves of air by which they are transmitted—that those separated by an octave, for instance, are in the proportion of 2 to 1, and it is also easy to imagine that a sound of any particular pitch is specially impressed upon some particular portion of this spiral membrane—just as in the case of two harps tuned in unison, the sound produced by one will set in motion the corresponding string of the other, and cause it to vibrate and to emit the same sound. The transverse fibres of the spiral membrane may be considered to act exactly as the harp, and, if this is the case, all that is wanting is that each nerve-fibre should receive an impression from the corresponding fibre of the spiral membrane, and convey it to the brain, to produce there the impression which we recognize as sound. This may readily be effected by means of the rods, the inner of which are surrounded by very delicate nerve-cells. This view of the function differs slightly from that of the author, but it may possibly be more correct.

This subject is one in which the writer has long taken a great interest, and having been strongly impressed with the correctness of the views above set forth has endeavoured, but hitherto without success, to construct some mechanical illustration of the mode in which a sound of any given pitch becomes impressed on the corresponding portion of the spiral membrane; but the very lucid description of the actual mechanism of the cochlea given by Dr. Pritchard will probably conduce to the accomplishment of this object.

With regard to sounds of very high pitch, the power of appreciating them in different ears is very various. This fact was well known to the celebrated Dr. Wollaston, who stated that he had two small pipes of very high pitch, and between which there was (quoting from recollection) an interval of a fifth, one of which he could hear and the other he could not; and he met with some who could hear the notes of both pipes, others could, like himself, hear only one, and others could hear neither of them. There are individuals (of whom the mother of the writer was an example) who cannot hear the shrill chirp of a cricket. These differences in the range of audition may be readily accounted for if it be the fact, as it is not improbable, that the narrowest portion of the spiral lamina may vary slightly in width in different cochleæ.

Immediately following the former is an important paper by Mr.

Wenham, reprinted *verbatim* from the 'Proceedings of the Royal Society,' on "A New Formula for a Microscope Object-glass." In this combination the front lens is a single plano-convex of unusual thickness, being, in fact, considerably thicker than a hemisphere; behind this is a plano-convex triple, the middle lens of which is a double concave of dense flint-glass; and behind this a plano-convex single lens, the plane side of which is uppermost. The focal lengths of these lenses are about in the ratio of 1 : 3 : 4·5. It may here be remarked that the diagram Fig. 6, representing the halves of this and a prior construction of Mr. Wenham's, is incorrectly shaded, and therefore rather misleading; *flint* and *crown* glass should be respectively *similarly shaded* on both sides of the diagram. In the absence of any verification of the projection of the diagram, the statement of Mr. Wenham respecting the paths of the extreme rays through the combination may, without doubt, be taken for granted. From this it appears that these rays are, on their final emergence from the posterior surface, brought into a state of parallel coincidence, in place of converging near the conjugate focus, as was generally the case with objectives of the old construction. An important advantage of this appears to be that more distant conjugate foci may safely be employed; or, in other words, that amplification of the image by elongation of the body of the microscope may be effected with less loss of definition than in objectives of the usual type. The writer has long since satisfied himself that with the objectives in his possession, comprising some considered to be first-rate by their respective makers, there is less loss of both light and definition in obtaining increased magnifying power by elongation of the body of the microscope than by the employment of very deep eye-pieces. Another advantage of the new formula appears to be that when the glass is in adjustment for covered objects, sufficient space remains for further approximation of the single front to the triple lens in order that an adjustment for immersion may be effected without any change of lenses. The writer has long desired an opportunity of comparing some objectives made on the new formula with those of similar power of the usual construction, but—perhaps because the demand for these glasses has been in advance of the supply, for it appears that the tact and delicacy of hand required for the manufacture of the minute lenses of objectives of high power is rather a question of intuition than of mere training—that wish has not as yet been complied with.*

* Since this statement was made, an opportunity has occurred of comparing several of the new objectives, ranging from $\frac{1}{4}$ to $\frac{1}{15}$ in. focus, with others of the old model; and it is unquestionable that they have no reason to fear comparison with the very best objectives of the old pattern. The only exception (*quantum valeat*) that can be taken to those objectives is that there is somewhat less flatness of field; but the writer cannot avoid expressing his conviction that in the objectives of the future, and especially in the higher powers, the new formula will supersede all those hitherto adopted.

It must be observed that in objectives of high power the reduction of the number of surfaces, some of which are of deep curvature and difficult construction, from 16 to 10, is of itself a very great advantage, since the scattering of light, and consequent fog, incidental to repeated surface action, must necessarily be much diminished. It appears very desirable that when the best form and dimensions that experience can dictate have been arrived at, the results, thrown into an algebraical form, should be submitted to the differential calculus, in order that it might be determined whether the results are the best attainable. To this object the writer would willingly render any assistance in his power.

Some observations upon the angular aperture of an object-glass by Mr. Tolles in air and water and in balsam, have also been contributed by Mr. Wenham as the result of an examination which was made by him in his study, at which the writer was present, and testified that the angles actually found were those which were stated. This has, however, been somewhat disputed by the maker, who claims a larger angle than that which was found. The lens in this instance was properly corrected as a dry lens, and then after measurement in air it was measured in water and then in very fluid Canada balsam without alteration of the adjustment. It may be quite possible that if the lens had been readjusted so as to give the best image for immersion in balsam, a slightly greater angle might have been obtained; but this would not have been a fair way of making a comparison, as it is not the mode in which the glass would ever be employed in actual practice.

Several papers of a more or less controversial character have been written by Dr. Royston-Pigott and Mr. Wenham on the vexed question of the Podura markings, about which so much difference of opinion has existed, and on other tests of optical definition; and it may here be remarked that in these, as in the discussion of all other purely scientific questions, anything approaching to acrimony is alike to be deprecated and deplored.

The introduction of a combination of lenses between the eyepiece and objective for the purpose of amplifying the power of any given objective was, it is believed, first suggested by Dr. Goring, in his 'Micrographia,' published about forty years ago; an arrangement of this kind constitutes the "Aplanatic image-searcher" of Dr. Pigott, of which the writer has been unable to find anywhere a precise description. No description whatever appears either in our own or the 'Quarterly Microscopical Journal'; but on referring to the paper by Dr. Pigott in the 'Phil. Trans.,' Part 2, for 1870, it is found to be thus described:—"A pair of slightly over-corrected achromatic lenses, admitting of a further correction by a separating adjustment, are mounted midway between

a low eye-piece and the objective, so as to admit of a traverse of two or three inches by means of a graduated milled head. . . . . The focal length of the combination may vary from $1\frac{1}{2}$ to $\frac{3}{4}$ of an inch." And on reference to the figure annexed to the paper, the two combinations appear to be of about equal power, that towards the objective being a double-convex crown, corrected by a double-concave flint, and that towards the eye-piece, a double convex corrected by a plano-concave; but the focal lengths of these compound lenses, their curvatures, and the distance between them are not given. The chief subject of examination by Dr. Pigott appears to be the image of an object with well-defined outlines, such as the bulb and lower end of the scale of a finely-divided thermometer; such image being formed in the focus of a deep objective, placed as a condenser axially with the objective to be examined.

The writer has hoped to have been able on the present occasion to bring before the Fellows some definite and intelligible principle of action of this contrivance, in order that some idea might be formed as to the mode of its action on the definition of certain test-objects; this he has essayed to do by means of both geometry and analysis, and he has moreover sought the assistance of an eminent mathematician of the most recent type; but he regrets to say that his endeavours have not been attended by any success. But he is far from desiring it to be inferred from this that no such definite principle is discoverable, as it may perhaps be the case that, since more than forty years have elapsed since he was a Wrangler at Cambridge, his mathematical weapons are too rusty to contend with the numerous phalanx of conditions arrayed against them. But it may be doubted whether Dr. Pigott is in possession of any more definite information than that which he has already made public; and even the name of the instrument, a "searcher," seems to imply that its application is wholly empirical: even in his own hands it has been observed that while the desired appearance is sometimes speedily produced, at other times a considerable amount of manipulation has seemed to be required for that purpose. Under these circumstances it appears that the controversy that has existed as to whether the beaded appearance of the well-known markings of the Podura scale is the result of improved definition, as it is affirmed to be on the one side, of some spectral illusion, as it is assumed to be on the other, must still be considered an open question.

The writer, reviewing this question under the dictates of common sense, when observing the familiar Podura notes of admiration well defined and free from colour, cannot resist the inference that in the objective all aberrations are nicely balanced, and the object truly represented in the visual image; on the contrary, when the same

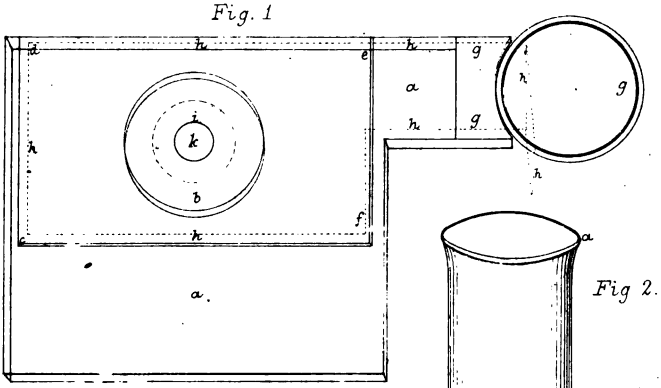
object is viewed as rows of ill-defined beads loaded with colours, it is difficult to avoid suspecting that the appearance is a spectral illusion, resulting from some unexplained diffraction or interference; and this suspicion can hardly be dispelled from his mind by anything short of rigid mathematical demonstration. In the 'Proceedings of the Royal Society' for June, 1873, an account is given by Dr. Pigott of the appearances presented by a Podura scale accidentally crushed by pressure, in which considerable tracts of the markings appeared to have been resolved into an assemblage of closely-packed spherules, and this appearance is assumed to confirm the reality of the beaded appearance above mentioned. But considering the widely divergent views that have been entertained by many competent observers as to the actual structure of the object in question, the result of such severe mechanical treatment can hardly be trusted as affording any satisfactory indication of structure; and having seen the markings standing out like fingers from the edge of an accidentally sliced or folded scale, it is difficult to resist the impression that the latter, viewed simply as the result of a mechanical injury, is much the more trustworthy.

In the number of the Journal for March last, Dr. Pigott has pointed out the source of some spurious appearances in the scales of *Lepisma Saccharina;* and the sentiment with which this paper commences may be cordially endorsed, namely, that "until spurious appearances are no longer accepted as true, it is impossible that definition in the microscope can arrive at that degree of perfection which is required by the spirit of the age." The author points out that some of the spurious appearances produced in this object may be obviated by cutting off, by means of a diaphragm with a small aperture, the more oblique rays of the illuminating pencil: it appears very probable that oblique illuminating rays are the fertile source of many delusive appearances that are met with in microscopic examination.

Your President having fulfilled the office of Scientific Juror at the Vienna Exhibition, it may be expected that some account should be given of the microscopes and objectives there exhibited. There are several reasons why a satisfactory account cannot conveniently be rendered. In the first place, the extreme apathy shown by English opticians with reference to the Exhibition led to the almost entire absence of English work, there having been in fact only one exhibitor of microscopes, Mr. Pillischer, and he, though established in London, is by birth a Prussian; and not even in his collection was there a single objective of note, or of high power. It did appear, therefore, that it would be hardly fair to compare objectives which were exhibited with others which were not. Another reason is that the time allotted to the jurors for the examination of objects and apparatus was very short; in fact, there was no time allowed for

anything more than a very superficial survey, whilst the means provided for enabling the jury to make such an examination were extremely meagre. Suffice it to say that of the objectives exhibited, those of Hartnack, Gundlach, and Nachet were decidedly the best. The immersion objectives of Hartnack performed very well, and his were, on the whole, the best; those of Gundlach and Nachet were exceedingly good, but not quite so good as those of Hartnack. It should perhaps be mentioned that almost the only test submitted was the fine markings of *Surirella gemma*, and these were certainly very well shown. With regard to the mechanical arrangements, the German type of stand generally prevailed, and this is so well understood that it is needless to say much by way of description. It appears to be wanting in many appliances which English makers furnish, and which are found very convenient; but, at the same time, for class instruction its steadiness and simplicity must recommend it to many students.

Fig. 1



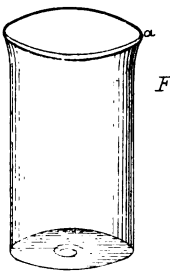




Fig 2.

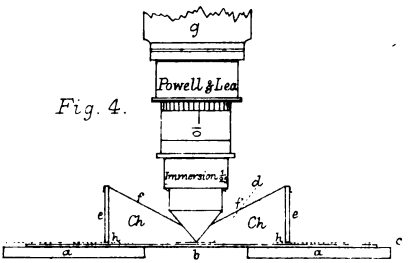



Fig. 4.

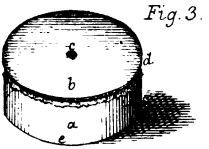



Fig. 3.

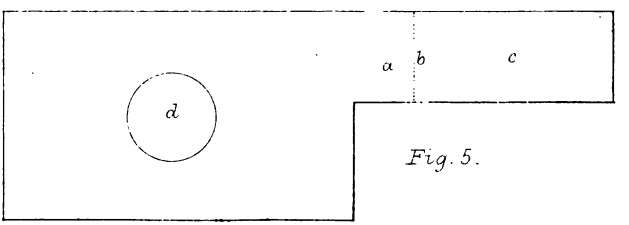
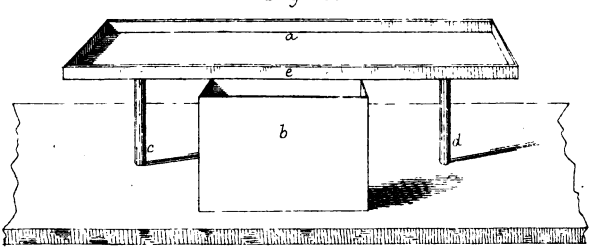



Fig. 5.

Fig. 6.



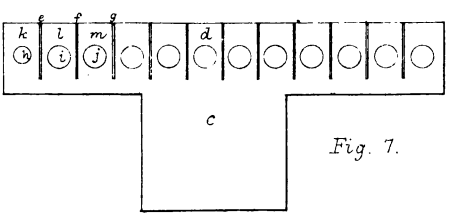




Fig. 7.

W West & Co. lith.

Life History of Monads.– *Methods of Research – Experiments on heat*

## II.—*Further Researches into the Life History of the Monads.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.

(*Read before the* Royal Microscopical Society, *Jan.* 7, 1869.)

Plate LIII., and upper part of Plate LIV.

*On our method of preventing the drop of fluid under examination from evaporating, so as to admit of continuous examination of the same forms with the highest powers.*

Recklinghausen's "moist chamber" only enables us to arrest for a short time the dissipation of the fluid under examination, and serves in practice only for low powers. The arrangement of Leuckardt, modified by Rindfleisch, only partially overcomes this difficulty, and presents others in working which made it incompetent for our purpose. But conserving what we perceived to be of service in both, and devising, as will be seen, other arrangements of our own, we were enabled to make an apparatus which proved entirely effectual.

It consists of a plain glass stage (*a*, *a*, Fig. 1, Pl. LIII.), so fitted as to slide on in the place of the ordinary sliding stage of a Powell and Lealand or Ross stand. It is thus susceptible of the mechanical motions common to those stages. Its foundation (*a*, *a*, Fig. 1) is plate glass, about the tenth of an inch thick, in order to give it firmness. But this is too thick to work through with a condenser and high powers; and therefore a circular aperture *b* is cut through it, and a thin piece of good glass (*c*, *d*, *e*, *f*) is fixed over it with Canada balsam. At the end of the arm *a*, which extends some distance beyond the stage to the *left* of the observer,* a brass socket with a ring attached is fixed with marine glue. It is marked in the drawing *g*, *g*, *g*. The object of this ring is to hold a glass vessel (Fig. 2, Pl. LIII.) about $1\frac{3}{4}$ or 2 inches deep. It simply drops in, and the top *a* being slightly larger than the ring *g*, Fig. 1, it is prevented from slipping through. Let us suppose the stage to be in its position on the microscope, and the vessel (Fig. 2) inserted in this manner into *g*, Fig. 1. A piece of good, new, and thick bibulous paper is now cut to the shape drawn in Fig. 5 of the same Plate; the part *a* being long enough to reach to the end of the glass stage, and then at *b* bend over, leaving the part *c* in the vessel (Fig. 2), which is inserted into *g*, Fig. 1. Its position is indicated in Fig. 1 by the dotted lines *h*, *h*, *h*, &c. But before it is laid upon the stage a circular aperture *d*, Fig. 5, is cut out, which must be much larger in diameter than the covering glass which it is intended to use. We therefore employ small

* In our case nearly 2 inches.

covers.* The vessel with the flap of blotting-paper in it is now filled with water, and a drop of water is placed on several parts of the paper, and the whole is very soon by capillary action thoroughly and evenly wet. A drop of the fluid to be examined must now be placed at *k*, and the covering glass *i* must be laid on. It will be seen that there is a broad clear space between the covering glass and the bibulous paper. We now want to form a chamber into which the object-glass can be inserted, and which shall enclose a portion of the constantly wet blotting-paper, and be to a very large extent *air-tight*. The consequence will be, that the evaporation within the chamber will be always greater in quantity from the blotting-paper, on account of its continual renewal, than it can be from the film of fluid. Indeed, the moisture in the chamber is so great under favourable circumstances, that it rather increases than allows a diminution of the film of fluid. The manner in which we effect this is simple. A piece of glass tubing, about $1\frac{1}{2}$ inch in diameter, is cut to about $\frac{3}{4}$ of an inch in length. At one end of this a piece of thin sheet caoutchouc is firmly stretched, and a small hole is made in its centre. Fig. 3, Pl. LIII., gives a drawing of it; *a* is the piece of glass tubing; *b* is the stretched elastic film, which is securely tied on by means of a groove in the glass at *d*; and *c* is the aperture. The bottom edge *e* should be carefully ground. This is laid in the position in which it is looked at in the drawing, on the blotting-paper of the stage, the aperture *c* being over the centre of the covering glass. The object-glass is now racked down *through the small hole c*, Fig. 3, and adjusted to focus. The caoutchouc should be thin enough to afford no impediment to the action of the fine adjustment; when it will be seen that it clasps the object-glass by its elasticity at the aperture; and the gentle pressure forces the under edge of the chamber upon the blotting-paper, so that little or no air is admitted; while if the under edge of the chamber be carefully ground it will suffer the stage, bibulous paper and all, to move *under* it when the milled heads for working the mechanical stage are in action.

A drawing of the apparatus in working order is given in perpendicular section at Fig. 4, Pl. LIII. The parts *a*, *a*, in this figure represent the glass stage *a*, *a*, Fig. 1. *b* in both figures stands for the round aperture in the thick glass. *c* in Fig. 4 corresponds to the thin glass which covers this aperture marked *c*, *d*, *e*, *f*, in Fig. 1. The blotting-paper is marked in dotted lines in both figures. *d*, Fig. 4, represents the covering glass *i* in Fig. 1; *e*, *e*, Fig. 4, is the piece of glass tubing *a*, Fig. 3; *f*, *f*, Fig. 4, is the stretched caoutchouc seen at *b* in Fig. 3, with the object-glass *g*, Fig. 4, penetrating and tightly filling up the aperture *c* in Fig. 3; thus forming the *moist chamber*, *ch*, *ch*, by enclosing parts *h*, *h*, Fig. 4,

* We find that the cover one quarter of an inch and the aperture in the blotting-paper $\frac{1}{16}$ths works well.

of the blotting-paper, which, from the glass vessel to the left of the stage,* is by capillarity always renewing its moisture.

It will be seen that the instrument must be horizontal; but there is no inconvenience arising from this if it be placed on a sufficiently low support; and it will be found in practice that it may be worked for a long time without any other change in the arrangement than the screwing up or down of the fine adjustment. The difficulties in working are few, and can be best discovered and overcome in practice.

### *Experiments on the Effect of different Temperatures on the Vitality and Development of the Monads described.*

Our method was to take a drop of the fluid containing the forms, and put it upon an ordinary glass slip, and cover it with a thin cover. It was then examined with great care, and a record made of what it contained and the condition the objects were in. It was then placed in the brass box *a*, Pl. LIV., upper portion, by taking off the well-fitting cover *b*. Two pairs of ledges inside enabled us to place a dozen or more slides within it. The cover had a suitable thermometer *d*, fitted in at *c* with cork, the bulb reaching exactly the middle of the inside of the box. Underneath this box a Bunsen's burner *e* was placed, with a series of jets in the form of a parallelogram, to correspond with the shape of the box.

A series of slips, having been carefully examined and inserted, and the cover of the box put on, heat was steadily and cautiously employed until the mercury stood at the desired point. It was then carefully maintained at this, in some cases ten and in others fifteen minutes. The box was then allowed to cool slowly, and when cold the objects were removed, and a drop of distilled water inserted by capillarity. Each slide was then immediately examined and reported upon. It was then inserted in a large moist chamber, to be watched regularly through succeeding hours or days. The moist chamber we devised is simple and effectual. It is drawn on Pl. LIII., Fig. 6. *a* is a tray standing on two legs *c*, *d*, and further supported by the trough *b*. This trough holds water. A piece of bibulous paper is cut to the size of the tray, but with a flap to go over the edge *e* and fall down into the trough *b*. This is the "bed" on which the slips are placed. Another piece of blotting-paper is now cut to the same shape and size, and then is further cut as shown in Fig. 7. *c* is the flap to go into the trough; *d* is the part that will lay on the "bed"; but it has pieces cut out, as at *e*, *f*, *g*; and beside this, circular apertures, considerably larger than the covering glass on the slip, are cut out, as *h*, *i*, *j*, &c. This

* The vessel will of course require refilling according to the rapidity of the evaporation; but we now purpose obviating this to a large extent by using a vessel formed on the principle adopted in the construction of the water trough of a modern aviary.

piece of paper is laid on the top of the other, and then the parts *k*, *l*, *m*, &c., being carefully raised, the slip may be inserted so that the blotting-paper covers it and yet leaves a vacant space all round the cover. If now a thick piece of plate glass be laid upon the top of the tray, a moist chamber is formed in which the slips may preserve the drop of fluid moist for an indefinite time. By constantly examining them we were enabled to report what changes might be ensuing from hour to hour, and when not being examined they were simply replaced in the chamber.

It will be seen that by this means no precautions were employed for the exclusion of extraneous germs or other organisms; but then none were needed. *We disregarded whatever else might appear besides the forms whose germs we knew existed in the fluid before heating; and we confined ourselves solely to the development of these. And on this subject no mistake could arise, for we had made ourselves masters of their life history.*

Of the three forms which we describe in our "Further Researches, &c.," it will be remembered that the first emitted palpable germs. The second was, to use a concise term, viviparous, emitting no germs but the cyst opened to give birth to minute living forms. The third, which we describe in this communication,† emits a sporule which is undiscernible by any powers we could employ. As we do not venture to name these forms, we will designate them as I., II., III., which indicates the order of our communications on "Further Researches, &c."; but as II. emitted living monads instead of spores, we will keep this before the reader by placing an asterisk beside the II., thus II.*

1. Six slides were taken, and on examination were found to contain all three of the above forms, and in almost every stage. These were placed in the heating box, and the temperature slowly raised to 82·22 C.; they were kept in this temperature ten minutes; then allowed slowly to cool.

On examination after moistening, nothing could be seen in the field but an amorphous mass. Not a trace of motion could be seen with the best light and 2500 diameters.

These were now placed in the moist chamber and examined at intervals, with the following results in eight hours and a half, *viz.*: On all six of the slides young of III.; on five of them young of I.; on *one* only of them young of II.* Subsequently drawings of development of each of these were made from these slides.

2. Six more slides were prepared from the same solution, and found on examination to contain I., II.*, III. They were kept in a temperature of 93·33 C. for ten minutes. On examining them after moistening, they presented the same aspect as before: no trace of motion, and no semblance of life. They were placed in the moist

† The former part of this paper was printed in the February number of this Journal, p. 69.

chamber. After regular watching for ten hours we were enabled to decide that the *young forms* of III. were found on four slides; the young forms of I. on three slides; but II.* were wholly absent. The remaining two slides were barren of result. These conclusions were confirmed by more prolonged research on the same slides.

3. Five slides containing I., II.*, III., kept in a temperature of 121·11 C. for ten minutes. On examination immediately after heating, nothing but a still and shapeless field, with no trace of life, could be seen.

They were placed in the moist chamber. After eight hours, minute moving points, which we did not venture to identify, were seen in *four* of the slides, and a minute form of III. in one. In the course of the next twelve hours, complete forms of I. and III. had developed, but none of II.* Nothing appeared in the other one; it was barren to the last.

4. Six slides, containing I., II.*, III., were heated up to 148·88 C. Results as before on first examination. After eleven hours a minute point had been followed into a young condition of III. (*vide* Fig. 16, Pl. XLVI.), and in the course of twenty hours the young of I. and III. had been followed to the adult stage or nearly so in two of the slides. III. followed in the same way in one other, and the other three yielded no definite results; development appeared to commence, but not continue.

These are only typical results of a larger series of experiments.

It will be seen, then, that the two forms which emitted sporules were able to survive, *by means of their sporules*, a temperature of 148·88 C., whereas the form which gave birth to minute *living* forms only feebly survived a temperature of 82·22 C.; while again, the form whose sporules were too minute to be seen appears to have slightly the advantage in the contest with heat.

We are not wishful to enter into the speculative aspects of this very important question, but we are tempted, in conclusion, to state briefly what commends itself to us as the most probable way of accounting for the above results.

We are not inclined to attribute to the vital element of the sporule the possession of any exceptional power, which will explain its ability to resist such high temperatures. We the rather believe that from some physical cause it has been prevented from encountering it, by protection.

The adult forms are undoubtedly destroyed at a temperature of from 61° to 80° C. It is not difficult to account for this. The sarcode in a perfectly fluid solution cannot escape rising to the heat of the fluid; for from the very nature of the active vital processes, a constant current of liquid must be passing into and out of the sarcode by imbibition and exmosis. It must follow that when the temperature is too high for the continuation of the vital processes, death must ensue. This temperature all experimenters concur in

placing within 80° C. Nevertheless, if there are any solid particles, such as cheese, or turnip, or viscid earth, or if the boiling is not so conducted as to secure an equal temperature throughout every part of the flask, some adults even may escape. With proper precautions, it has been shown by Cohn, Horwath, B. Sanderson, and even by Bastian and Huizinga themselves, that this may happen.

But as to germs or sporules, we have now shown as a matter of fact that they do resist a heat which destroyed not only adults, but immature forms of those species that are born with the vitality of the sarcode in complete action. How can this be explained? Is it reasonable to suppose that in bodies so inconceivably minute there can be special arrangements for heat resistance which are not found in the adults? There is no argument that we are acquainted with that makes this supposition impossible. Their size, it is true, must be less than the $\frac{1}{200000}$th of an inch, even where visible, for they are only barely so to the $\frac{1}{50}$th. But what is this, after all, to the minuteness of the molecules of matter? According to Prof. Tait, "In a single drop of water there are a thousand quadrillions of ultimate particles. Each particle in a drop of water is to the entire drop as the size of a walnut is to the earth." Now the molecules of living matter are undoubtedly extremely more complex and larger than those of water. Yet it has been shown by the experiments of Davaine,* and confirmed by Clementi and Thin,† that the living particles which produce septicæma can carry on an infinite multiplication of their numbers and cause the death of the animal into whose blood they are inserted, and that in a quantity only corresponding to the 10 trillionth of a drop. Between this and our sporules the $\frac{1}{200000}$th of an inch there is surely "ample room and verge enough" for a complicated protective structure, which should have the power to resist the diffusion of heat, at least for a time. For the germ may be emitted with its sarcode in a fixed state, not having any interchange of fluid with the surrounding medium, and thus differing wholly from the adult or immature viviparous forms.

What may be the nature of this protecting envelope or medium it is at present impossible positively to say; but it will be remembered that in every case in which we have seen a monad-sac emit sporules, it has *always been in what we can only describe as a "glairy fluid"*; and when we remember the remarkable effects of the spheroidal state of water, we must be more than cautious in denying that such protecting envelopes may be formed in such a medium by analogous means. Indeed, we have the guide of facts in this matter, as the results of experiments on the desiccation of rotifers will serve to show.‡

* 'Comptes Rendus,' 1872.
† 'Edinburgh Medical Journal,' July, 1873.
‡ 'M. M. Journ.,' vol. ix., 201.

But this protection, whatever it may be, is only for *a time;* for all authors concur that if the boiling or heating be continued long enough, with proper precautions, all living things, germs as well as adults, are destroyed. This is attested by Wyman, Cohn, B. Sanderson, Roberts, Pasteur, Huizinga, and others. Only a long series of carefully-conducted and controlled experiments on forms thoroughly worked out, can enable us to decide what length of time is absolutely fatal to the sporule as well as the adult. At present we believe that any results are vitiated by taking it for granted that they must be destroyed at an arbitrary temperature endured for an arbitrary time. Who is to say, without competent experiment on the form (whose life history should be known), that more prolonged heating would not have yielded other results?

But this is only a negative error. A much more serious one, we believe, has been introduced, or at least endorsed by Dr. Bastian, when he assumes that the prolonged heating, or high temperatures we ask for, must destroy the *organizable* constitution of the albuminous matters which were spontaneously to form into bacteria. That anything is or can be "organizable" in any sense except as being capable of assimilation as pabulum is not even *ex hypotheci;* it is simply and entirely *petitio principii;* and we know as a matter of fact that these substances, after being exposed to heat enough to destroy living organisms, are nevertheless quite fit to act as pabulum to other organisms, and are thus "organizable" in the only *known* mode in which any matter ever becomes living.

Our researches show conclusively (we hold) that the assumption that the germs of putrefactive organisms must perish in the same conditions that destroy the parents, is erroneous. We have not shown this of bacteria, it is true—the germs may be so minute that our present optical appliances wholly fail to detect them—indeed, the facts shown by us in relation to the monad we describe in the earlier part of this paper * strongly support this. But it must be remembered that the monads we describe are as much putrefactive infusoria as the bacteria, and with as great a title to be called "spontaneous" as they have. Scientifically we can see no reason why the bacteria should be held to be more likely to grow spontaneously, than any other of the less minute forms whose life history our present appliances enable us to work out. We venture to think it more philosophical to work by experiment *down* to the bacteria, hoping that by an accumulation of facts concerning these minute forms, and probably—as we may anticipate—improved optical appliances, to deal with the development of bacteria as we deal now with the development of creatures more amenable to our knowledge and our instruments.

* 'M.M.J.,' Feb. 1874, p. 71.

## III.—*Further Notes on the Zoosperms of Crustacea and other Invertebrata.* By Alfred Sanders, Lecturer on Comparative Anatomy at the London Hospital Medical College.

(*Read before the* Royal Microscopical Society, *Jan.* 7, 1874.)

Lower portion of Plate LIV. and Plate LV.

Since writing my last paper * on this subject, I have not failed as opportunity offered to examine the zoosperms of various invertebrata, and more especially those of crustacea; the result has been to confirm the conclusions at which I then arrived, and to bring to light many kinds of zoosperms as they vary in detail in different species. The genus Pagurus seems to offer a great variety of forms, every species as far as I have examined them differing from every other in the proportion of the caput to the rays, or of the latter to the tail, or again in the shape of the head.

In *Pagurus maculatus* (Fig. 1) the zoosperms have a more elegant shape than in *P. Bernhardus*. The contour of the head is concavo-convex; swelling out at the base it becomes concave as it contracts towards the point. When they are viewed from above the outline is seen to be circular. It appears that the cortical part is composed of a different material from the centre, for on the application of sea-water the latter portion is entirely forced out and destroyed, leaving only the outer shell. The rays are three in number; they are flex-

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EXPLANATION OF LOWER PART OF PLATE LIV. AND PLATE LV.

Plate LIV.

Fig. 1.—Zoosperms of *P. maculatus*; *a*, side view; *b*, top view; × 600.
„ 2.—Zoosperms of *P. maculatus* not quite mature × 600.
„ 2 *a*.—Larger cells from the testis of *P. maculatus* × 300.
„ 3.—Zoosperms of *P. callidus* × 600.
„ 4.—Zoosperms of *P. ornatus* × 600.

Plate LV.

„ 5 *a*.—Smaller particles of protoplasm from testis of *Porcellana platycheles*.
„ 5 *b*.—The line dividing the same into two unequal parts.
„ 5 *c*.—Smaller section beginning to bud out in three stages.
„ 5 *d*.—Mature zoosperms; all about × 770.
„ 6 *a*.—Spermatophore, front view, of *P. platycheles*.
„ 6 *b*.—Portion of the basal ribbon.
„ 7.—Zoosperms of *Galathæa squammifera* × 770.
„ 8.—Zoosperms of *Palemon squilla* × 700.
„ 9.—Zoosperms of *Epeira diadema* × about 900.
„ 10.—Zoosperms of *Phalangium cornutum* × 600.
„ 11.—Zoosperms of *Asteropecten crenaster* × 600.
„ 12.—Zoosperms of *Holothuria tubulosa* × 600.
„ 12 *a*.—The capita of the zoosperms of *H. tubulosa* after they have imbibed sea-water × 600.
„ 13.—Zoosperms of Aphrodite sp.? × 600.

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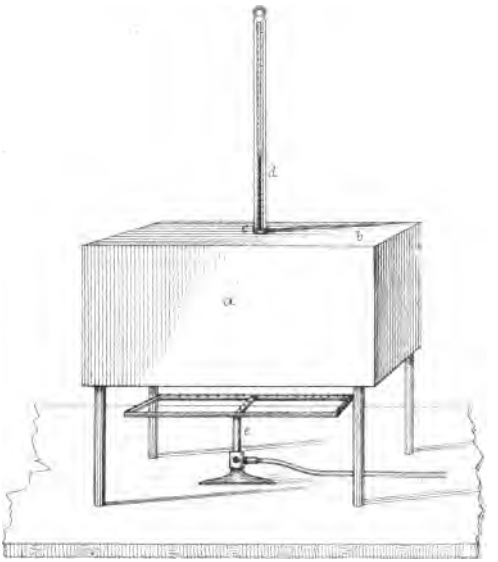
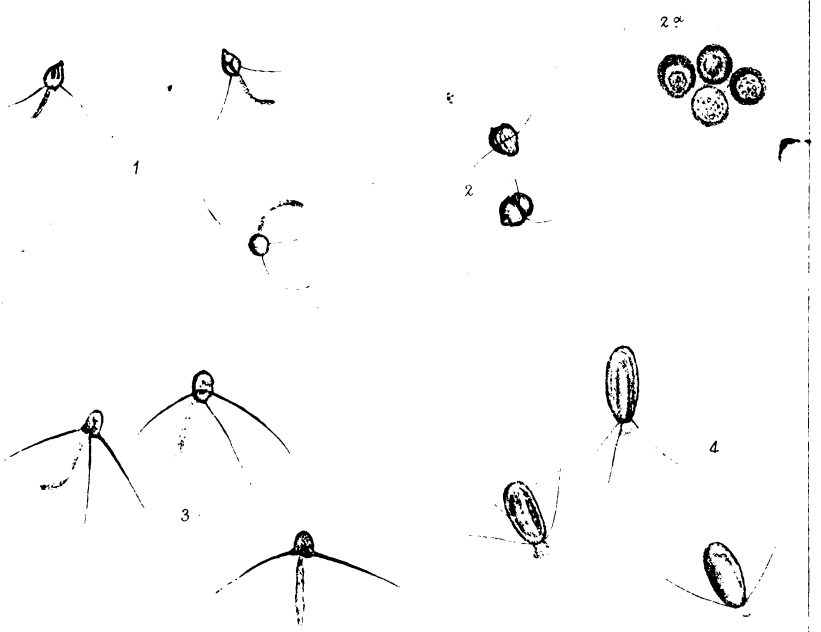
* 'M. M. J.,' May, 1869.




*Life-History of Monads. — Experiments on temperature.*




The Zoosperms of Crustacea.

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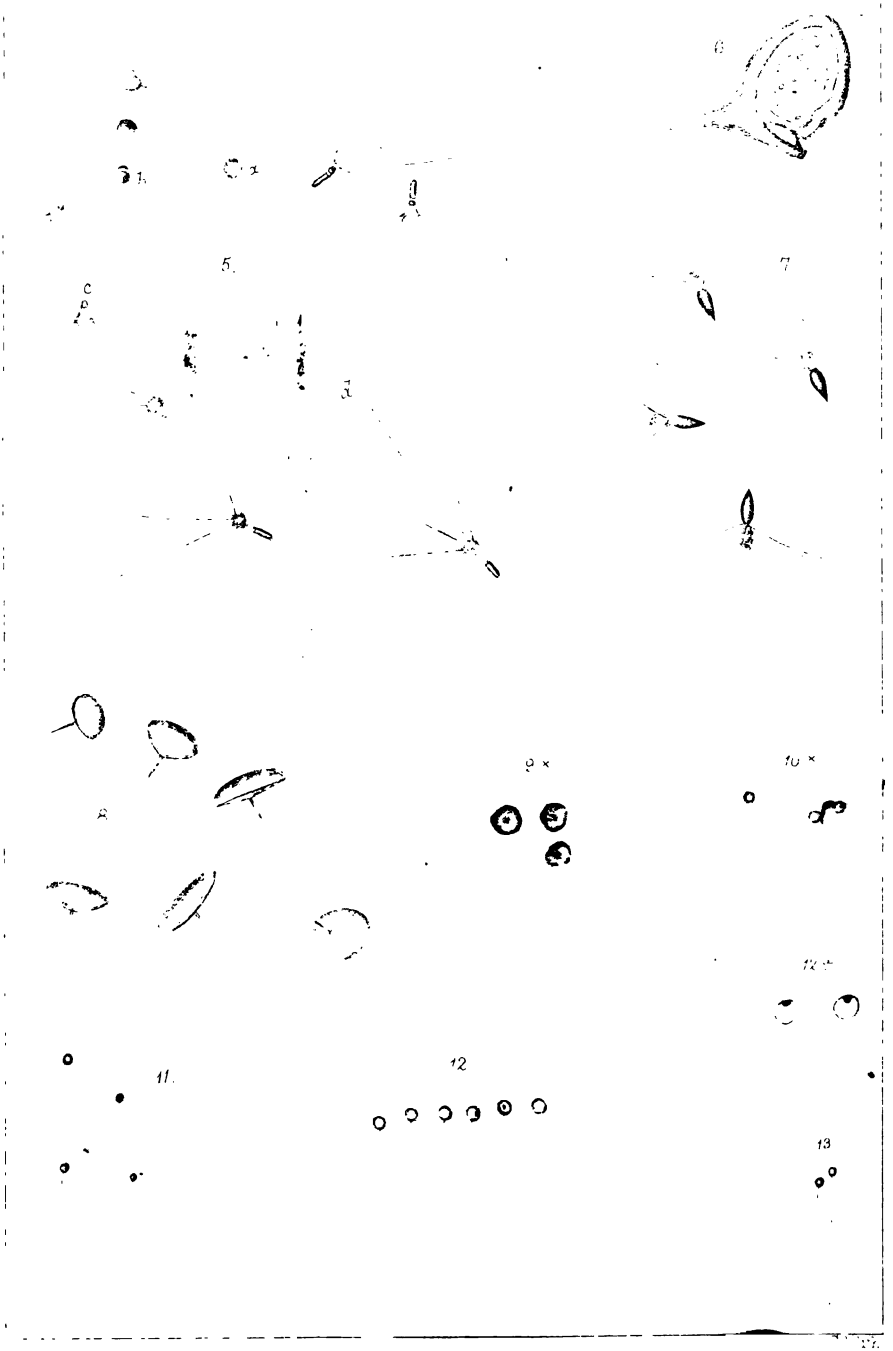



The Entosperms of Crustacea

ible and pointed: the sarcodous part or tail stretches out for more than the length of the caput, and consists as usual of a very finely granular protoplasmic material; it is thick and undivided, not presenting the branches which were found in *P. Bernhardus*. The length of the caput is generally about 0·006 mm., the breadth about 0·004 mm.; the rays measure about 0·016 mm. in length, while the tail is 0·008 mm. The stages of development in this species resemble those in *P. Bernhardus*. Fig. 2 gives a representation of zoosperms which are not quite mature; the tail has not yet appeared; the rays are shorter, and the caput is larger than when they arrive at their full growth. The animal itself was very plentiful in the Bay of Spezia, and many specimens presented an interesting instance of commensalism; some of them inhabited a shell much too small for them, forming in fact but a *point d'appui* for the end of the abdomen, the rest of their protection being afforded by a dense red sponge, which externally formed a rounded mass shaped like a pebble, and internally was excavated into a smooth passage adapted to the shape of the crab. Both these animals had evidently grown old together, and the hermit finding the sponge to form such an efficient shelter, had not thought it worth while to change its abode, as is the custom with the rest of its congeners.

Another small species of hermit crab from the same locality, which I take to be *P. callidus*, is provided with zoosperms, the capita of which are more robust than those of *P. maculatus*. The contour is usually dome-shaped; but sometimes it approaches to a quadrangular form with the anterior angles rounded off. Across the base there runs a ridge of refracting material which makes this part square in some cases, while in others a portion of the softer substance which appears to occupy the centre projects beyond the ridge, which then does not form the limit of this part of the zoosperm. There are three sharp and flexible rays, as in the last. The tail or soft appendage varies much; in some specimens it is short and thick, in others it is long and thin, while others present every gradation between the two extremes. In this species the appendage looks like a thick walled bag attached to the base of the caput, composed of the same kind of material as in the *P. maculatus*. Fig 3 gives a representation of them. In size they are a trifle larger than those of *P. maculatus*.

In *P. ornatus*, another small species which I found at Leghorn, the zoosperms are gigantic in comparison with those of *P. callidus*, *P. maculatus*, or *P. Bernhardus*, and there is nothing in the size of the animal to account for this difference, as it is much smaller than the latter species, and no larger than the two former. This shows that zoosperms, like blood corpuscles, have no relation to the bulk of the body. In shape the zoosperms in this species are oval, in some cases with the sides slightly flattened. When first placed on the

slide, they appear homogeneous, bright, and highly refracting; but when left for a very short time a clearer longitudinal space appears in the middle line at the end to which the rays are attached; in the centre this is transformed into a dark, club-shaped rod. The rays are extremely fine, and in many cases scarcely perceptible; they generally have about the same length as the caput, and are usually turned forward; the only appearance of a tail is a slight square projection at the narrower end; after a short time has elapsed this is superseded by a sort of effusion of extremely fine sarcode resembling that of infusoria, which forms a cloud-like spherical projection. After the zoosperms have been dead some time, nothing remains of them but the framework, which shows lines forming an oblong, with a button-like projection internally at one end. This form is represented in Fig. 4, and presents some points of resemblance to the zoosperms of the common lobster. The length of these bodies ranges between 0·012 mm. and 0·018 mm., and their breadth is about 0·006 mm. and 0·007 mm. The spermatophora exactly resemble those I have described as belonging to *P. misanthropus*,* to which species the present one is nearly allied.

*Porcellana platycheles* is a very common crab, found under stones on our coast just above low-water mark at spring tides. Its zoosperms differ a good deal from those of the Paguri. In the typical specimens (Fig. 5 *d*) the caput consists of two parts, and has the appearance of a rod with a knob at one end. The anterior extremity of this rod is occupied by an oblong mass of highly refracting material, while the remainder of it, together with the knob itself, is made up of a finely-granular nearly homogeneous sarcode, which I might have imagined to be homologous with the tails of the zoosperms of Pagurus, were it not that the rays are derived from this part instead of from the highly refracting part. They arise from the knob, and are generally four in number, two on each side of a conical projection in the mid line. The shape of this sarcodous part varies considerably in different specimens; in some it becomes flat, and more or less like a disk; in others the highly refracting part is simply capped by a small portion of sarcode which runs out into the rays without forming any portion of the rod. In some cases the refracting part is broken, a small portion being divided from the remainder and lying detached in the sarcode. The total length of the caput varies from 0·006 to 0·011 mm.; of this the refracting part occupies 0·004 mm. This part is nearly always of the same length, the difference in size of the zoosperms being generally caused by the sarcodous material. The rays, as a general rule, equal about $3\frac{1}{2}$ times the length of the caput; they become of such extreme tenuity at their extremity that it is very difficult to measure them.

* *Loc. cit.*

This species gives a very good illustration of the mode of development of these bodies; they could be traced from small spherical particles of protoplasm, which could not be called cells unless the absence of both investing membrane and nucleus be excluded from the definition of that word; these particles divide into two unequal parts by a line across; the smaller part continues to protrude until the whole looks like a round cell with a bud attached to it; this bud gradually assuming a substance more compact than the other part of the protoplasmic mass, becomes rounded and more elongated; the rays in the meanwhile bud out by degrees from the larger section, the remainder of which forms the sarcodous part of the mature zoosperms. These particles measure about 0·004 mm. in diameter, and correspond to the smaller cells * in *P. Bernhardus*. Other bodies occur which exactly resemble the larger cells in that species. Between the two forms every gradation in size is found, but I could not trace any other connection between them. In the testis the larger cells occupy the outside and extremities of the sacculi, the smaller particles fill the remaining portion of the sacculi, and the mature zoosperms are diffused through the centre of the testicular tube. This arrangement looks as if the relation between the two forms was one of genesis; be that as it may, the zoosperms themselves do not begin to be evolved except from the smaller particles. In Fig. 2 *a* is given a representation of the larger cells from the testis of *P. maculatus*, which exactly resemble both those of *P. Bernhardus* and those of the present species.

The spermatophora in this animal are attached in an irregular manner and in close apposition to a broad elastic ribbon which occupies the whole length of the vas deferens, so that it appears that they all get into the female organs in one mass. In a front view they are seen to be broadly lanceolate; viewed on the side they are lenticular. (Fig. 6.) They consist of a thick outer envelope, which is attached by its base to the ribbon; and an inner thin oval structureless membrane, forming a closed sac which contains the zoosperms; the length of the spermatophore, excluding the base, is about 0·034 mm., and the breadth is 0·031 mm.

The zoosperms of *Galathæa squammifera* (Fig. 7) somewhat resemble those of *P. platycheles*, but are of a more elegant shape, and differ also in the point of attachment of the rays. The caput is of an elongated lanceolate form; the representative of the tail is an irregular mass of granular matter attached to the caput by a narrow neck. The caput appears to be formed of a smooth, homogeneous, highly refracting substance, devoid of those internal markings which are apparent in the zoosperms of the Paguri and other crustacea. The rays are attached close to the neck of the granular

* *Loc. cit.*, Plate XI., Fig. 10.

substance, which thus projects between them; they generally about equal the two parts of the zoosperms in length, and therefore measure 0·012 mm.

Kölliker * gives a figure of the zoosperms of *G. rugosa*, which differ considerably from those of *G. squammifera*. The spermatophora also, according to the same author, are curious, being arranged on a stalk, either branched or simple like the leaflets on a pinnate leaf.

*Palemon squilla* (Fig. 8) presents a type of zoosperm differing from the last, and more nearly approaching that of Stenorhynchus and Maïa; they consist essentially of a disk, from the centre of which projects a sharp pointed rod. In those zoosperms which appear to be of the normal form, one side of this disk is flat, and is bordered by a substance of greater consistence than the remainder; this substance is thicker in the middle than at the sides, and from its centre springs the rod, which appears to be composed of the same material. The part of the disk on the side opposite the rod is convex, and is composed of a soft protoplasmic material resembling that which forms the knob-like portion of the zoosperms of *P. platycheles*. When first examined this part looks quite smooth, but it becomes very granular after remaining some time on the slide: the whole disk has somewhat the shape of a saucer. On obtaining a top view, the rod is seen to be implanted into the disk by means of a forked base, which forms an eminence in the centre, the space between the two forks being filled with a granular matter.

The *Palemon squilla* has no spermatophora, but the distal part of the vas deferens is filled by a very tenacious elastic substance, which appears to resemble in its properties that material which forms the base of these bodies in *P. Bernhardus* and *P. platycheles*, except that it has not become so much consolidated. The vas deferens of the lobster is filled with the same kind of substance.

The forms of zoosperms in crustacea appear to be strictly regulated in accordance with the external shape of the animal. All crabs with a square or round carapace that I have examined follow the type of *C. Mænas*, and seem to vary only in the number of rays. Seen on a top view, for instance, the zoosperms of *Pilumnus hirtellus* have three rays, while those of *C. Mænas*, when seen in that position, present so many, that it is extremely difficult to determine whether they really exist or not, the usual appearance being that of a circlet of granules surrounding the zoosperm, like the nimbus round the head of a mediæval saint. When we come to those crabs which have a triangular carapace, like Maïa or Stenorhynchus, we find another type. In the Paguri, and those forms which mark the transition between the Brachyura and the

* 'Ann. Sciences Naturelles,' 2me Série, tome 19, 1843.

Macroura, almost every species differs from every other in the form of the zoosperms, yet they may be all referred to the same type. In the Isopoda and lower crustaceans, forms which as yet I have not much examined, the zoosperms are linear, resembling those of other classes in the animal kingdom. Now, if the facts of nature mean anything at all, all crustaceans are derived from one primitive form, as Fritz Müller * has shown to be extremely probable; and it becomes interesting to inquire how, as the external forms of the crustaceans became modified, the zoosperms became modified also, for it is evident that these changes of the external form were simply the indications of more profound variations going on within. Natural selection does not appear to me to be competent by itself to effect this, neither do I believe in the single law which is supposed by some authors to impel species to vary in some definite direction; rather, I think, it must be due to a collocation (to use a phrase rendered classical by the late Mr. John Stuart Mill) of most intricate laws acting and reacting on each other, and which, in the present state of our faculties, it seems impossible to analyze, but which might perhaps be made more plain by an illustration. A cod-fish, for instance, as is well known, lays some thousands of eggs; of these eggs some, say a dozen, grow up to maturity: none of these thousands of eggs and young fish perish by chance, but their mode and time of dying depend on an immense number of circumstances, such as hereditary disposition, greater or less abundance of enemies, heat, cold, or density of their surrounding medium, and thousands of other factors of which we know nothing. In some such manner as this do the laws of nature combine to alter the constitution of animals and form new species. Natural selection itself is but a metaphorical term applied to a certain fraction of these laws.

In addition to the zoosperms of crustacea I have examined those of a few other invertebrata; among them were specimens of the *Epeira diadema*. In these spiders the zoosperms (Fig. 9) are reduced to great simplicity; they consist of a round body, which possesses no appendages whatever in the way of rays or tail. When first placed on the slide these bodies appear quite homogeneous, but after a short time a circumferential portion separates in appearance from an internal portion; on the addition of water, or even after they have remained some time on the slide, they burst, the internal portion disappears, the external portion remains as a spiral or curved thread, somewhat resembling a very short linear zoosperm; in such a condition it appears that Leydig † saw them, for he has given a figure of the zoosperms of Epeira which presents a certain likeness to these filaments. That this thread is

* Für Darwin, translated by W. S. Dallas.
† 'Lehrbuch der Histologie,' fig. 261.

an artificial and not a natural appearance is easily seen by applying a small quantity of water, when the bodies in question begin to swell up, a distinct thread appears round the circumference, they burst, and this thread alone remains as evidence of the previous existence of a zoosperm.

The zoosperms of *Phalangium cornutum* (Fig. 10) resemble those of Epeira in being simple disks without any appendages; they are circular, and have very thick highly-refracting borders with a bright centre. Although resembling the zoosperms of Epeira in form, they must have a different composition, as they do not burst either from remaining on the slide or from the addition of water.

I have not had much opportunity of investigating the zoosperms of the Echinodermata, but I have examined one species of Starfish and one Holothurid. The Starfish, *Asteropecten crenaster*, possesses zoosperms of the linear form (Fig. 11). The caput is a spherical body, which varies in size in different specimens; in the centre there is a more or less bright spot: in diameter the disk measures 0·002 mm., sometimes more, sometimes less. The tail is of excessive tenuity, and generally measures about fifteen times the diameter of the head. After the zoosperm has remained some time on the slide the caput swells up, and takes on the appearance of a vesicle, having a highly-refracting somewhat square particle placed on its circumference, which particle appears to be the remains of the bright spot which occupied the centre of the caput.*

*Holothuria tubulosa*, a species which is very common about the shallow parts of the bay of Spezia and at Leghorn, has zoosperms somewhat resembling those of the Asteropecten, but larger and more active. The head jerks from side to side, often without making any progression; the vibration is so extremely fast, that the caput frequently appears double; at other times they advance violently, and then suddenly stop; others make excursions in a circular or serpentine course; altogether they give one more the impression of independent animals than organic units; such motions have even caused writers to attribute volition to monads. The caput of this species of zoosperm (Fig. 12) consists of a globule of highly refracting matter, with strong thick walls which are dark, and in the centre there is a bright spot. The caput generally measures 0·004 mm. in diameter. The tail is long and of extreme tenuity, and it averages from ten to twelve times the diameter of the head; but the measurements of such mobile and fine lines are so difficult that they can scarcely be depended upon for exactitude. Like the zoosperms of Asteropecten, these imbibe fluid after death, and take the form of a vesicle, to the wall of which is attached a small rounded highly-refracting particle, the tail having disappeared (Fig. 12 *a*).

* Sea-water effects this change much more quickly.

The single species of Annelid that I have been able to examine, Aphrodite sp.? has zoosperms very much like those of *A. crenaster*, but smaller, presenting a globular body, with a slight tendency in some cases towards a pointed oval shape. The centre is occupied by a brighter spot (Fig. 13). The caput measured about 0·002 mm. The tail, which, like that of the Holothuria, is of extreme tenuity, measures about thirteen to fifteen times the diameter of the head. The motions of these zoosperms are not so lively as those of the latter species. Sea-water also acts more slowly on them, but after a time they expand and become mere transparent vesicles when placed in that fluid, but there is no appearance of the refractive particle adhering to the circumference. The zoosperms in this animal differ less apparently from those of the starfish, which belongs to another class, than do the zoosperms of one species of crustacean from those of another and nearly allied species; and indeed it is curious to note the varieties in the shape of these bodies, and it would be interesting to find out the cause. The Ostracoda for instance, as described by Dr. Zenker,* have most enormous zoosperms having the appearance of a twisted rope, but creating the impression in one's mind that they must be spermatophora.

The subject offers a wide field for research for those in want of an occupation for their microscopes of more physiological interest than perhaps even the markings of diatoms; and although much has been done by men like Kölliker in the general subject, and incidentally to their special studies by Claparède and Quatrefages, yet more remains for future explorers.

* Wiegman's 'Archiv f. Natur Geschichte,' 1854, Jahrg. xx.

## IV.—*Angular Aperture of Object-glasses.*

By F. H. Wenham, Vice-President R.M.S.

If useful facts are brought to light by the aperture question, I presume that it must continue to be one of interest. I appear again thus early, not in a captious spirit of contradiction, but to notice some fallacies in the methods hitherto used for measuring the aperture of both dry and immersion lenses. The conditions carefully arranged would have been worth bringing before the Royal Microscopical Society, had they not been developed by controversy both strong and irregular, in which no Society could properly be involved, and consequently the question must be settled in the same way.

The controversy was begun by myself three years ago; I then disputed what I considered to be an erroneous theory, appearing in an essay illustrated by large diagrams, to prove an increase of aperture alleged to be obtained by immersion lenses,* and pointed out how rays were taken from wrong positions in impracticable constructions. The author of the essay referred to had merely carried the rays into imaginary front lenses, and there deserted them, regardless of their ultimate destination to a posterior conjugate focus at the eye-piece. The subject has been kept up at intervals by correspondents, with whom I have still to deal. It is difficult to do this without exciting some degree of irritation, because the nature of such strictures implies ignorance of the laws of optics — a science which has long been so exactly defined that there should be no error of the passage of a ray through refracting surfaces.

According to disposition, so do modes of controversy differ: perhaps my own is an obnoxious one. Let it be compared. Some always write in the first person singular, and reiterate their views as if to command belief, conceding no credit, and ignoring all replies except to those who favour their assumptions, with an air that says—

"I am Sir Oracle, and when I ope my mouth let no dog bark!"

This scarcely brings forth fruit. I am always glad to exchange notions with practical working men, and to give my own in ordinary phraseology.

Then there is discussion with a snarl, of which this aperture question affords some examples. This is the least productive of any, for its main strength consists merely in picking out contradictions and anomalies of phrase; science is tossed aside, and its cold reasoning avoided because it is not understood, and abuse is mistaken for keen argument. Satire may appear in discussion, arising

* 'M. M. J.,' p. 16, vol. iii.

not from ill-temper, like the last, but it is apt to offend. I find it difficult to restrain the propensity at times, though quite aware how few can accept it. Again, there is the meek and amiable style, that can neither make or answer a strong objection, saying, "I am sorry that I have disturbed you, gentlemen; I will drop the question rather than disagree!" This I cannot take credit for, but judging from the attitude of some of my opponents, it would seem as if I am a wolf amongst the lambs.

This prologue may afford material to those who find it more easy to comment upon words than scientific facts; it, however, precedes an announcement that I have to make, that object-glasses have at length arrived at 180° of aperture. A country correspondent in this Journal once asked the question whether "the old science of optics was now played out, or if the world has taken a turn the other way for a change?"* Can this be? a position at which simple teaching has told us *nothing* can be seen, and yet here is a thing said to be seen where it cannot exist. I had the glass in my hands, and rubbed my eyes in vain, but there it was, beautifully engraved, "R. B. Tolles, Boston. Immersion $\frac{1}{8}$th, 180° balsam, angle 98°." Yes! ONE HUNDRED AND EIGHTY DEGREES!

Having recovered my surprise, I proceed to tests. My remarks have only to refer to the question of aperture. The object-glass belongs to Mr. Crisp, a gentleman well known for his liberality and freedom from prejudice in the pursuit of microscopical science, and it is understood that the maker or sender is willing that this peculiar property of aperture should be commemorated.

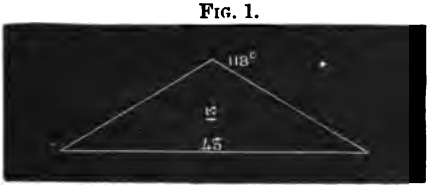
The object-glass was first tried on such tests as required large aperture for their determination. I could glimpse striæ of equal difficulty on the same object in Möller's proof slide, with another object-glass of 120°. Is all aperture beyond 120° then useless? I next measured the Tolles $\frac{1}{8}$th in the ordinary sector with lenses quite closed. *Light was seen* up to 180°. There was no definite margin at any point, for disappearance was gradual. Then came the question of how much of this light belongs to aperture in relation to diameter of front lens and focal distance.

The diameter of front lens to edge, as measured by micrometer, was ·043 of an inch. If anyone had asked me what diameter would be required for 180°? not wishing to exaggerate (as some might argue that even space must have its limit), I should have said not less than the crown of your hat. Therefore a diameter of ·043 for an angle of 180° is a marvellous accomplishment. If I had also been asked what focal distance could be got with 180°? my answer would have been ·000, but the actual distance in this lens was ·013, a most comforting length for 180°.

* 'M. M. J.,' March, 1873, p. 124.

Mr. Tolles has admitted that his judgment is not warped by any theory. I must, however, trouble him with a little—a very little —not more abstruse than that the combination of two quantities produce a third, or that three points make a triangle. Let us take, then, a diameter line of 43, and a median height of 13, and we have here our angle, Fig. 1, 118°, all that the object-glass *can* take in by carefully-ascertained dimensions. Thus, in this case, from the fact of plain measurement, an aperture beyond 118° is impossible.

Fig. 1.




Were it merely my purpose to point out errors, I might now stop; but there is no benefit to the community at large unless the relations of cause and effect are investigated. The appearance of excessive aperture has always been a vexed question with me, having seen numerous instances where extreme rays form no image. On looking obliquely with a magnifier through the front lens of an objective of very large aperture, the image of a flame becomes much elongated or distorted towards the margin, and at last the boundary is a mere indefinite ring of light. I have not believed this to constitute true aperture, which must mean image-forming rays only.

Fig. 2.

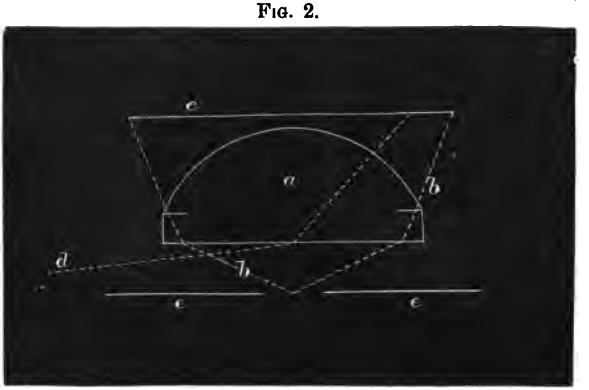



Diagram Fig. 2 will demonstrate how rays beyond the true aperture can enter the back lenses so that light may be seen up to the last degree:—*a*, front lens of object-glass; *b b*, rays forming a true limiting aperture of 130°, and entering middle lens of the series on line *c*. Take a ray, *d*, incident on front surface at 5° (equivalent to 170° aperture). This ray enters the back lenses as false light, but comes to no focus, or forms no image. If a small stop or narrow

slit *e e*, with knife edges, is placed before the object-glass, when the aperture is measured it will cut off all false rays, and confine the image-forming ones to a definite margin. The focus of the object-glass must be exactly in the plane of the front of stop. All rays up to 180° can then reach this point, and consequently all true ones within such an angle will be admitted. In this way the angle of Mr. Tolles' $\frac{1}{8}$th was measured.

In order that no after-quibble might be raised concerning the position of the adjusting collar, this was set to the closest point of the lenses, thus giving the utmost obtainable angle. A brass disk was turned, ·013 in. thick (the exact focal distance of the object-glass). In the centre of this disk there was a conical opening, the diameter of which at the base was much beyond that of front lens, and at the apex $\frac{1}{50}$th of an inch, coming to a sharp edge. The disk was blackened with perchloride of platinum, and was attached to front of object-glass as follows. It was laid on a glass slip on stage of microscope, with the small end of aperture downwards; the top side was touched with two opposite minute dots of liquid gum. The object-glass was now brought down gradually, keeping the aperture exactly in the centre by means of the stage movements. This could be done with the greatest nicety; when the object-glass reached the disk this became attached, and scratches on the glass slip were in focus.

Though the opening at the small end of the aperture was only $\frac{1}{50}$th, it did not encroach upon the full field of view. There is no risk of injury to the object-glass by the operation, as it outspans and covers it. The object-glass with its attached disk was now tried for angle with the usual sector, still with the lenses at closed point. Instead of 180°, the aperture was at once shewn to be the more rational and wholesome angle of 112°, only *six degrees* less than the diameter of the lens could possibly admit from the point of focus. With this arrangement the margin of light in the eye-piece was very clear and definite.

The balsam aperture had now to be ascertained. It is evident that the same source of error from an unshaded front must arise, and is perhaps more likely to occur, for the reason that the diminished incidences on the surface of the lens facilitate the entrance of false light beyond the angle of true aperture, therefore the shield or cone leading up to the focal point must still be used. I have advocated the tank method for taking immersion apertures, because it requires no focussing or niceties of manipulation, by which errors of accident or design may be attributed, but it is both messy and inconvenient; I therefore did not adopt it. In some degree this objection applies to Col. Woodward's plan, in which the balsam should be somewhat opalescent, in order to trace distinctly the cone of light that pervades its substance, and with a high-power

object-glass the margin is so feeble as almost to forbid the measurement of its boundary.

I first tried the plan described by myself many years ago. I got a block of flint glass of the same refractive power as hard Canada balsam. This I had no difficulty in finding (the specific gravity was 3·1). Fig. 3, is the glass, 2¼ inches wide; the side *b* was polished, and the opposite one, *c*, greyed and divided into degrees. As the divisions were on a line, they were of course irregular; but this caused no difficulty, as they were lined off from a dividing circle. Half-way down the exact centre of polished edge there was a fine diamond cut, from which point the divisions were lined, and on which the object-glass to be tested for immersed aperture was focussed. As the refraction of the glass is the same, it must represent the balsam angle, which can be read off by the extent of the light on the division scale of degrees. It made no difference in the angle whether water is admitted in front lens or not (as in theory it should not), because the medium is in the form of a parallel plate, but in each case the object-glass must be focussed on to the glass surface. The measurements obtained this way were on too minute a scale to be very reliable. I therefore adapted a semi-cylinder of glass, of density 3·1 and 1¼ diameter, with polished edges, such as has been used in the instrument for demonstrating the law of Descartes. The centre of radius was exactly in the polished plane, and this point indicated by a line ruled by a diamond, upon which the object-glass was focussed. A minute stop of leaf metal was placed in the centre, so as to cut off extraneous rays. The semi-cylinder was fixed in the stage of a microscope which had a rotating base divided into degrees. The body was set horizontally, and a lamp placed some two feet away. The index was set with a half-luminous field, and the object-glass focussed carefully on to the glass surface. Unless this is done there will be considerable error in the measurement. The microscope is next rotated, and when the light again bisects the field, the degrees are read off. By these means the balsam aperture *with closed lenses* was only 68°, instead of 98° as stated; and this angle of 68° was the same whether water was introduced or not between the plane surface of front lens and semi-cylinder, taking care to focus in either case. The principle of operation was identical with that of the ordinary sector, to the moving index arm of which the hemisphere may be supposed to be fixed, and the focal front of object-glass *a*,

Fig. 3.

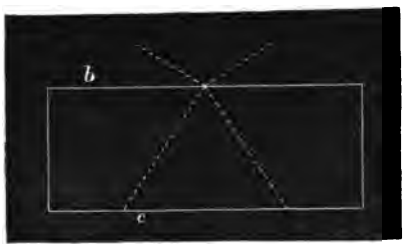
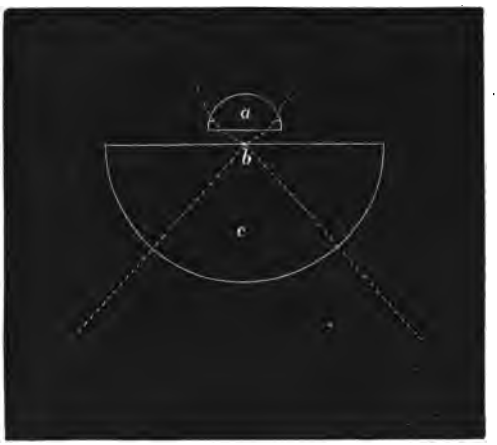


Fig. 4, brought in to the centre, *b*, of the semi-cylinder *c*, at which there is a thin metal slit or stop of suitable diameter.

FIG. 4.




In this measurement I have rather over than under estimated the aperture from using a stop too large; less than $\frac{1}{50}$th of an inch would have been more proper.

Believing that apertures, to be correctly measured in future, must have all extraneous lateral rays cut off by a slit or stop of very thin foil *in the focus* of the object-glass, and as this requires to be an adjustable piece of apparatus, the following plan has been arranged.

In order to prevent injury to the edges of the slit, and to keep them in one plane, the two strips of foil (which may be of platinum) are cut with a knife along a steel straight-edge. One of these pieces is cemented on to a glass slip, the other is stretched across a perforated metal plate sliding on the top of the glass, and adjustable by a screw. The edges of the two strips abut against each other, and are adjusted for parallelism by setting up the edge on the glass to the other while the cement is warm. The instrument resembles an eye-piece micrometer. The glass surface serves to focus upon, by which the coincidence with the anterior plane of the slit is ensured. The thickness of glass beneath will not alter the angle of aperture, as this is the same both for incidence and emergence. The arrangement is placed on the microscope stage, which should be a very thin one; the slit is adjusted to the proper width, and brought centrally in the field, and the object-glass focussed on to the glass surface, with the microscope in a horizontal position, and the lamp flame a foot or two away. I have an instrument rotating from a centre pin in its own base, which is divided; but in lieu of this it may be set on a small wooden turn-table divided at its edge.

The aperture is then measured in the usual way. If the slit is very narrow the limits of aperture will be known by the sudden disappearance of light. If it is opened wider so that the edges just appear in the field of view, the boundary of the circle of light limiting the aperture may then be seen to bisect the centre; it is therefore preferable to open the slit till the edges appear in the margin of the field.

It may not be necessary to fit up an arrangement for balsam apertures, because this must always follow a *true law* relative to the other, and cannot exceed 82° in ordinary object-glasses, to be also used dry, notwithstanding all that has been alleged to the contrary (for my case is not disproved, my friends, and I have not yet "come off second best").

I will now briefly refer to the plan whereby I first obtained the full dry aperture on an object *immersed in balsam*. Firstly, the whole aperture primarily exists in the object-glass proper; the hemisphere cemented over the object had nothing to do with any increase optically as a question of refraction, for it exerted no refraction whatever. Whether the sphere was of longer or shorter radius made no difference, provided the object lay in its centre. It acted in either case for a *radiating* pencil, just as a piece of plane glass acts for a *parallel* beam, and neither magnified or diminished. The combination was not strictly an immersion object-glass, for the hemisphere might be said rather to belong to the object, and as part of it, forming together a dry element with a structure in its centre. To make it an immersion, in the true sense of the term, insert water, or say balsam, of the same refractive power as the hemisphere, between it and the front of the object-glass proper, and *then* it becomes a simple immersion, with its correspondingly reduced aperture. With balsam the hemisphere is absolutely nil in effect, which remains the same, whether it is there or not.*

As my opponents hold trigonometrical demonstration so lightly, I will recapitulate the conditions in somewhat unscientific terms.

* The experiments with an additional hemispherical lens set over the objects were tried by me many years ago for the purpose of ascertaining its practical value for the exclusive investigation of balsam-mounted objects requiring large aperture (as an addition for improving a properly-corrected *dry* lens it is useless). The same lens with the object in the centre of radius would serve for any object-glass in which there was sufficient distance before the front lens to introduce it. Eighths and upwards of large aperture come so near that it is scarcely possible to make any use of it, as an extra adaptation, suitably mounted in another setting in front. With a $\frac{1}{2}$th or $\frac{1}{4}$th not much exceeding 100°, there is ample space. Finding the effect but little superior to that produced by a higher power in the ordinary way, I did not trouble the makers about adopting the plan, which I should have done had it proved of sufficient utility. I was then aware of the optical conditions involved, and published them at the time, and did not abandon it without due experiment. I am stating the candid fact, not with the view of disparaging any present attempt, though this in all probability will now be the motive attributed to me.

Given an object-glass of extreme available aperture, place a plate of parallel glass in front, although the focal point is carried farther off, yet the ultimate aperture beyond the glass remains the same. Insert water between the two, and the focus is still further extended, but the final maximum aperture remains as before, and is neither increased or diminished. Now, with the object-glass the same as at first, set the focus on a balsam-mounted object; the angle of the radiant pencil from this for extreme emergence from the slide *must* be limited within the maximum of 82°. After refraction, the rays enter the front of object-glass, and this identical law of refraction *again limits the internal angle to* 82°, *the same as in the slide.* Now introduce fluid between the two (say balsam as before); the focus becomes extended, by which the bend that existed between the two dry surfaces *is drawn out straight*, if I may so term it, and the lines of similar angle that existed in the slide, and correspondingly in the front lens, are now unbroken, and continue entire from the radiant point to the back surface of front lens.

In the early part of this controversy, it was contended that the whole aperture could be obtained with an immersion lens of ordinary construction on balsam objects. Even with the utmost stretch that opposition can favour, there has been a great reduction on this assumption, and with the gross errors that may arise, and to which I have now called attention, in the measurement of all extreme apertures, I maintain that no aperture consisting of image-forming rays beyond 82° can be seen, if measured properly and with freedom from prejudice, in an ordinary object-glass without additions, and that can also be used on *dry* objects, or such as are not mounted in balsam, which include the majority of tests.

## V.—*Further Remarks on Immersion Apertures.*

By J. J. Woodward, Assistant-Surgeon U. S. Army.

I am glad that Mr. Wenham finds "a real pleasure" in discussing this question with me, since I have somewhat more to say, this time in the shape of comment on the subject of his "reply" in the December number.

In the first place, I might easily cite passages from his former papers to justify my understanding that he distinctly denied the possibility of constructing "an object-glass with an immersion aperture exceeding 82°," but I willingly admit that I misunderstood his meaning, since he now says this was not his intent, and that he at once concedes "the full aperture in an immersion system specially designed for the purpose as that was."

In the next place, I cannot assent to his implied claim that he was the originator of four-combination immersion objectives possessed of a balsam angle greater than 82°. In the experiments on which this claim is based, the hemispherical lens was united to the cover by balsam, and entirely detached from the objective. If the idea that the nearly hemispherical *front of an objective*, united to the cover simply by water-contact, might play a similar role in transmitting pencils greater than 82° from a balsam-mounted object, ever occurred to him before I described the device of Mr. Tolles, in my paper last June, he has certainly not put it on record.

Even now he seems still unwilling to believe the possibility of the transmission of a pencil greater than 82°, if the objective has but two posterior combinations, although he sees that it is easy enough if it has three. I am glad, however, to perceive that he is almost persuaded, for he writes: "The capability of taking in a few extra rays may depend upon the form and size of the back lenses," which is substantially what I urged in my November paper.

To the argument of that paper he makes, if I fully understand him, but one objection, and that, I must say, appears to me to involve an essential oversight. I refer to the first paragraph on page 257, in which he objects to my assuming successively two different focal points for the same front, saying that "if F is considered right, F′ must be wrong." Now, surely, no one knows better than Mr. Wenham, although he appears to ignore it altogether, that the distance of the focal point from the front of an objective with a screw-collar is not a fixed quantity, but has a maximum and minimum, with an infinite number of intermediate values. Every time the screw-collar is turned the fine adjustment of the microscope has to be moved. A separation of the two posterior combinations from the front enables the objective to be used with a lower magnifying power and angle; an approximation of the posterior combinations to the front enables it to be used with a higher magnifying power and angle than is obtained at the intermediate positions. Of course, if the object viewed is uncovered, the posterior combinations can only correct the aberrations of the front in one position, which is recorded on the screw-collar as the "uncovered point"; when the screw-collar is turned from this position a covering glass of suitable thickness must be introduced. It is clear, therefore, that to say "if F is considered right F′ must be wrong," without knowing the construction and position of the posterior combinations in the case under consideration, is to make a mere assumption for which there is no foundation, and which is not strengthened in the least by the equally unfounded assumption that "the first position forms a posterior focus, the second does not."

In the paragraph following this dogmatic assertion he asks me a question, which, perhaps, I do not rightly comprehend, for it is not

expressed in his usually clear style; I will therefore state what I suppose to be asked before I reply. I understand him to call attention to the familiar fact that no ray coming from behind, which arrives at the front surface of an objective at an angle of incidence greater than the angle of total reflexion from glass to air, can emerge *into air* in front of the objective; and then to ask me whether I have "observed any such limit *when dry* to be exceeded when the front is immersed?" To which I reply that in every one of the objectives I have examined, which has a greater balsam angle than 82°, this is the case of course. Surely Mr. Wenham must have known when he asked the question, that even in immersion objectives "with an additional front," with which he concedes a greater angle than 82° can be obtained "in the body of the front," that angle will still be rigidly limited to 82°, if air be substituted for water in front of the objective.

I repeat what I have asserted from the very first, that I give my unqualified assent to the proposition that 82°, or double the angle of total reflexion from crown glass to air, cannot be exceeded "in the body of the front of a dry lens"; but I also point out that precisely the same optical laws which fix this limit for the dry front, would fix 122°, or *double the angle of total reflexion from crown glass to water*, as the limit for the immersion front. That the opticians have not yet succeeded in constructing immersion objectives of this extreme angle is not because such a pencil cannot be transmitted by the front, but because they have not yet learned to correct its aberrations. That up to 100° the aberrations may be corrected successfully, is shown by the objectives of Mr. Tolles, which I have described.

Mr. Wenham further asks of me—"Can he show us the passage of the rays through one of the object-glasses, such as he advocates, in a diagram of correctly enlarged dimensions?"—referring, I suppose, to objectives of three systems only, since the case of the others is conceded. I reply that, if Mr. Tolles thinks proper to deviate from the ordinary practice of those who make objectives for sale, and to communicate for publication the details of the construction of either or both the objectives described in my November paper, it will be an easy matter for me to gratify Mr. Wenham, and I will endeavour to do so. Not that I think this additional testimony needed to show the accuracy of my measurements of the immersed apertures of these objectives, but because, besides the value of the information to objective makers, I should be happy to be the means of adding to the scanty store of facts which the objective makers have placed at the disposal of science. Mr. Wenham may be justly proud of what he has done in this direction, but he must not be surprised if those who make their living by constructing objectives are less liberal than an amateur can well afford to be.

In concluding his article Mr. Wenham devotes a paragraph to the defence of his mode of measuring the angle of Mr. Tolles' $\frac{1}{10}$th, which requires a few words. It now appears that this immersion objective was understood by Mr. Wenham to be a *dry one*. No wonder he found that a dry objective of his own construction, made twenty-two years ago, "proved far superior to Mr. Tolles', made three years ago, on every object on which it was tested." * Nor can I pass by the question of the position of the screw-collar used in that trial, without calling attention to a fact which appears to have been overlooked. Mr. Wenham says: "I should have been quite content to try it, if the adjusting collar had been pinned fast by the senders in any position," &c., &c. Now, in Mr. Tolles' original letter to Dr. Henry Lawson about this objective, published in England four months before the trial,† he indicated the proper position of the screw-collar to obtain the maximum angle of the objective in what ought to have been as positive a way as if the collar had been pinned fast. His words are—"Adjusted at 10½ closed from the open point (which is the intermediate point of the whole adjustment), I make the angle in balsam," &c.

Now, as I have said in my last paper, I utterly repudiate all accusations of bad faith against either Mr. Wenham or the gentlemen who were associated with him in this unfortunate procedure, but they certainly did not act as if they appreciated the importance of the position of the screw-collar, and, in consequence, their results have no significance as a check upon those of Mr. Tolles, and, indeed, no scientific significance whatever, as they did not state at *what* position of the screw-collar they measured, but simply measured at some undetermined position, selected, as it now appears, by trying to find at which an immersion objective would work best dry on a Podura scale.

Finally, I must allude, briefly, to the letter by the Rev. S. Leslie Brakey, in the December number, on the subject of my last paper. This gentleman, who is more than usually acrimonious in his language, finds no better way of defending his position than by misrepresenting both his own remarks ‡ and my reply.§ I do not think it necessary to do more than to request the candid reader to compare the version of both, in his present effusion, with the original articles.

War Department, Surgeon-General's Office,
Washington, D.C., *December* 30, 1873.

* This Journal, January, 1873, p. 32.
† Ibid., September, 1872, p. 148.
‡ Ibid., August, 1873, p. 99.
§ Ibid., November, 1873, p. 216.

## NEW BOOKS, WITH SHORT NOTICES.

The Preparation and Mounting of Microscopic Objects. By Thomas Davies. 2nd Edition (enlarged). Edited by John Matthews, M.D., F.R.M.S., Vice-President Quekett Microscopical Club. London: Hardwicke.—This little book, which has in our language no rival whatever, has made its appearance in a new edition, the former one having been about ten years in existence, and being hence somewhat behind the time. And in its new form it contains more than fifty pages of entirely new matter, and has an additional chapter by the present Editor, Dr. Matthews, who in it explains *seriatim* the various novelties in mounting and otherwise preparing objects, which have been devised both in the countries of our own language and upon the Continent; and indeed, for our own part, we consider this chapter of the greatest value to the amateur, who has frequently insufficient knowledge of languages to enable him to consult German or French authorities; and even if he had, has no opportunity of perusing these books themselves. For this reason we think, too, that the editor would have done well had he introduced a chapter on the subject of immersion lenses, and explained fully the mode of using these objectives, their several prices, and the best mode of procuring them. We think, too, that he would have done better had he introduced more matter on the subject of the preparation of purely anatomical objects. We fear that on account of this absence from the work of special advice on the mounting of the several specimens, which alone interest the medical student, the book will not appeal as fully as it ought to a very large class—now in fact a special society—of microscopic workers.

But in all that refers to the wants of the ordinary workers at the microscope, the book will be found amply full, and that too of really useful materials; for we find that both author and editor have been careful not merely to collect together facts, but to discriminate so that only the useful hold a place in these pages. We may mention a few of the authors quoted in this volume, to show how the editor has taken pains with his work. There are Dr. Beale, Mr. T. K. Parker, Dr. Carpenter, Dr. Klein, Dr. Alcock, Dr. Lockhart Clarke, Dr. Bastian, Mr. L. G. Mills, Mr. Edwards (New York), Herr Hyrtl, Mr. McIntire, Professor Williamson, Mr. Moseley, Mr. Dancer, Mr. Suffolk, Mr. Hislop, Mr. T. G. Rylands, and many others. The chapter on polariscopy, too, exhibits a great improvement on that in the former edition. The author has given fully and clearly the necessary information on this interesting section of microscopic work. We should have wished the authors had the sundry references to either of the Microscopical Journals more accurately given. When the work was first published, but one Microscopical Journal existed; but now there are two, so that a reference to the *Microscopical* Journal leaves the reader absolutely no clue as to which of the two magazines is referred to. We are, indeed, once referred to under our proper title, but that is all that we have been able to observe. And now, if we have found fault,

it must not be supposed that we have further complaint to make. We have cited all the points to which we object, but not a tittle of those of which we cannot but approve; and that we think is enough to show what an immensely improved edition is that which has made its appearance under the skilful care of Dr. John Matthews.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Pathological Changes in Cattle-plague.*—Dr. E. Klebs describes the results of his researches in this direction in the 'Würtzburg Verhand Phys.,' vol. iv., and Dr. Klein embodies them in his report to the 'Medical Record' as follows:—"The epithelium of the summits of the fungiform papillæ becomes disintegrated by immigration of micrococci from the surface; the blood-vessels and lymphatics of the mucosa become gradually filled with micrococci; these latter being regularly distributed through the mucosa, the cells of which commence thereby to proliferate. In the very resistant epithelium of the hooked papillæ there appear after a diffuse infiltration with micrococci a system of communicating cavities, which contain at first only lumps of micrococci, but afterwards, in consequence of a transudation from the blood-vessels, also serum and lymphoid cells. The ducts of the labial mucous glands become the seat of a very abundant accumulation of micrococci, which probably penetrate into the surrounding connective tissue and blood-vessels, thus causing in the former proliferation, in the latter emigration, of the cellular elements. In general the first changes take place in the epithelium of those parts of the mucous membrane of the mouth which are favourable to the adhesion of the food, *e. g.* the edges of the gums, the summits of the papillæ clavatæ and circumvallatæ, as well as the basis of the hooked papillæ, and these changes seem to be produced by the immigration of micrococci from the surface; *viz.* from the adherent food. In the mucous membrane of the intestine an infiltration with lymphoid cells takes place in the mucosa between the Lieberkühnian crypts, whereas the sub-mucous tissue and its blood-vessels contain abundant micrococci; in the latter they may produce even complete obstruction."

*Striated Muscular Fibre.*—Singularly enough an immense number of researches has been made on this subject within a comparatively short time, and a great many different observers have been at work on the same subject. Dr. Klein, however, has gone fully and fairly into the subject in two numbers of the 'Medical Record,' as the following lengthy quotation will show, which deals with the inquiries of Flögel, Merkel, Engleman, and Mr. Schäfer, whose researches we have already described:—

According to J. H. L. Flögel, in a paper on the striped muscles of mites,* the muscular fibres of the limbs and mouth, as well as those

* Max Schultze's 'Archiv,' vol. viii., part 1.

of the trunk of some species of *Trombidium*, are remarkable for the extraordinarily large distance of their transverse striæ, this being from ten to three micromillimètres. If the whole animal be placed for one or two hours in a solution containing 1 per cent. of perosmic acid, and, after having been washed in water, be dissected in diluted glycerine, the striped muscular fibres are seen under the microscope to have become stained by that reagent very differently in their different parts. First of all, the whole fibre is seen to be divided by transverse membranes or septa, continuous with the sarcolemma. Each division contains in its central region an anisotropous transverse disk, which appears to consist of a matrix slightly stained with perosmic acid; in it are imbedded rods which are stained readily by that reagent. In many cases, the transverse disk appears to be divided into two by a very slightly stained median layer, which probably corresponds to the median disk of Hensen. Each division also contains, between the above-mentioned transverse disk and the transverse membrane, an isotropous transparent disk, not at all stained by perosmic acid. This contains, more or less near to the transverse membrane, a row of granules of equal diameter, each of which represents the continuation of a rod of the transverse disk above mentioned; this row of granules is the granular layer of Flögel.

Besides these typical appearances of muscular fibres with broad striation, there are other fibres in which the transverse striæ are much narrower. On comparing these with the former, it is seen that the narrower the striation the more indistinct the median layer becomes, until finally it disappears completely, so that then the transverse disk consists of continuous rods; further, that at the same time the granular layer approaches the transverse membrane to such a degree, that they can hardly be distinguished as two different structures. Consequently the muscular fibres with narrow striation present the appearances commonly described, *viz.* a dim anisotropous disk; on each side of its short diameter (namely, that corresponding to the longitudinal diameter of the muscular fibre) a transparent bright layer: and finally, the bordering transverse membrane.

In polarized light (strong light and a magnifying power of 1000 being used) it is evident that, besides the sarcolemma and the transverse membrane, only the rods of the transverse disk and the rod-granules of the granular layer are doubly refractive, whereas all other parts are isotropous. From this it may be deduced that each muscular division is filled with an isotropous transparent matrix, in which are imbedded anisotropous rods; to each of these belongs at each extremity an anisotropous granule. In preparations made with perosmic acid, bundles of muscular fibres are not unfrequently met with, which, being fixed at one side by their insertion to the skin, show at the same distance from that insertion a fusiform swelling. This on close observation corresponds to a contraction-wave, rendered permanent by the perosmic acid. In such swellings, it is seen that the granular layer and the isotropous disk on each side of the transverse membrane have, together with this latter, become reduced to one broad layer which is even darker than the anisotropous transverse

disk, the rods of which have become shorter; they are reduced to two-thirds of their original length in the centre of the wave.

To preserve preparations, Flögel recommends the following method. The whole *Trombidium* having been kept for one or two hours in a solution of 1 per cent. perosmic acid, is placed in diluted alcohol, which in the course of several weeks is gradually replaced by strong alcohol. After this, the object is transferred into turpentine, and is finally dissected in solution of Canada balsam.

Dr. Merkel writes on the striped muscle in Max Schultze's 'Archiv,' vol. viii., part 2. He uses for his investigations the muscles of the thorax of the fly and the bee, either in the fresh condition after *rigor mortis* has appeared, or oftener after hardening with alcohol of 50 or 100 per cent.

Each muscular fibre appears to be divided into a number of transverse divisions, each of which is bounded at its extremities by a terminal membrane closely united with the sarcolemma. Through the middle of each division a membrane stretches transversely across; this corresponds to the median disk of Hensen, and is also fixed to the sarcolemma. The terminal membranes of two adjacent divisions are united by a thin intermediate substance, so as to form apparently one disk—the terminal disk of Merkel. These parts represent, so to speak, the framework of the muscular divisions. Each of the divisions is filled with transparent fluid substance, in which lies accumulated, at the sides of the median membrane, the gelatinous contractile substance, the proper transverse stripe; it falls away gradually towards the terminal disk. In the state of contraction, a remarkable difference in the appearances just stated is to be noticed; the terminal disk becomes thinner, the contractile substance leaves its former place at median membrane, having accumulated close to the terminal disk; consequently, each muscular division contains now one half of the contractile substance at each of its extremities. In the state of contraction, the contractile substance is darker than in the state of rest.

When a muscular fibre contracts, the individual divisions do not pass at once from the state of rest into that of contraction, but they go first through an intermediate state. This latter is characterized by the disappearance of all optical differences, the contents of the muscular division being perfectly homogeneous and at the same time very bright. This intermediate state represents, according to Merkel, the clue for explaining the above-stated displacement of the contractile substance; for during the intermediate state the contractile substance, imbibing all the fluid that is contained in the division, swells so as to fill out this completely, and, having gone through this preparatory state, again presses out this fluid, and accumulates on both ends of the division beside the terminal disk. The individual particles of the contractile substance press as close as possible to the terminal disk, the former trying to come into contact with the latter by as many of its particles as possible; in consequence of which the muscular fibre not only becomes broader, but also the fluid which is pressed out by the contractile substance is now accumulated in the middle of the division at the sides of the median membrane. As far

as volume is concerned, the contractile substance is in excess over the fluid in the state of rest, whereas in the state of contraction the contrary is the case.

In polarized light, and under high magnifying powers (600–800), it is to be noticed that the contractile substance as well as the terminal disk, is anisotropous; whereas all other parts are isotropous. In the state of contraction, the median membrane remains dark between crossed Nicol's prisms, it is therefore to be regarded as isotropous. In the intermediate state the whole fibre appears anisotropous, by which it is proved that during this state the contractile substance fills out the whole division.

In a second memoir in the same journal (vol. ix., part 2), Merkel occupies himself chiefly with the appearances presented by the striped muscular fibres in polarized light during the different stages of rest and contraction. From these observations, he finds that the appearances of polarization cannot be studied successfully on intact muscular fibres, but only on very thin parts of them; and, further, that the straining of muscular fibres with logwood produces effects which are similar to those observed in polarized light. These observations have perfectly affirmed the assertions made by Merkel about the structure of the muscular fibre and the displacement of the contractile substance during contraction.

T. W. Engelmann describes his microscopical observations on the striped muscular tissue in Pflüger's 'Archiv,' vol. vii., part 1. He studied the striped muscular fibres of arthropoda. They were observed in a moist chamber without the addition of any reagent, for reagents produced very marked changes. The fibres were in a perfectly fresh and living condition, showing still very lively contractions.

Each muscular fibre is divided into a number of divisions of equal sizes by transverse dark membranes—intermediate disks, which are closely united with the sarcolemma. Each division contains in the centre a bright, slightly refractive transverse median stripe—median disk of Hensen, on each side of which lies a dim, highly refractive band—the transverse disk; then comes on each side a bright, slightly refractive band—isotropous substance; then a dark, highly refractive stripe—the lateral disk; and finally, again, a thin, bright, slightly refractive band—isotropous substance; so that each division contains between the two intermediate disks, one median disk, two transverse disks, then two isotropous bands, two lateral disks, and finally, again, two isotropous bands.

(*a*) The intermediate disk, or the membrane of Krause, is distinctly to be recognized as a separate structure in the perfectly fresh fibre in the state of rest, when examined without a reagent, and if the height of a muscular division exceeds 0·008th part of a millimètre. In those cases where the lateral disk is very dark, and is in close contact with the intermediate disk, this latter may easily escape observation; it can, however, be brought into view by slightly stretching the muscular fibre. The intermediate disk appears under the microscope as a single dark line, being a homogeneous, highly refractive mem-

it is very elastic, and, when observed in polarized light, in ions which have been hardened in alcohol or perosmic acid, nted in dammar, distinctly anisotropous.

The isotropous thin band being at the side of the intermediate in fresh fibres only recognizable when the height of a muscular amounts to 0·008th of a millimètre and more. Otherwise the disk seems to be in contact with the intermediate disk. In er case the isotropous band can be brought into view by adding cent. of acetic acid, which causes the isotropous band to swell be perceptible.

The lateral disk is in the fresh fibre always darker than the ous band; it is seldom homogeneous, commonly granular. anules are generally of equal size, and isodiametric in such a at, where the muscular contents are divided into fibrils, each represents a part of a fibril. The lateral disk is not very ly anisotropous; it is also not so elastic and not so closely ed with the sarcolemma as the intermediate disk.

The isotropous band between the last-mentioned stripe and sotropous transverse disk is always easily to be recognized in ing fibre. Its thickness stands in a reverse proportion to that lateral disk. A 2 per cent. saline solution, water, or very l spirits, causes at once this isotropous band to turn dark. heated up to 50° Cent. (122° Fahr.) it becomes opaque and rm, but finally it shrinks. It is not a fluid substance, but con- f a number of soft granules of equal size, which are so much n that they touch each other completely; the number of these es corresponds to the number of fibrils into which the muscular ts split up occasionally.

In the fresh living muscular fibre, the dim, broad transverse pears to be divided into two by a median bright homogeneous erse band. In some cases, however, the latter is not to be made a separate structure. Between crossed Nicol's prisms, both the erse disks and the median disk are seen to be anisotropous. If living muscular fibres be treated with a 5 per cent. saline n, the transverse disks become swollen and pale, whereas the l disk becomes darker and narrower. Diluted acids and alcohol o 60 per cent. have a similar action. Heating brings out the l disk and the transverse disks also, as different structures.

len a muscular fibre dies spontaneously, or is subjected to the ce of water, diluted chromic acid, alcohol, corrosive sublimate, ne anisotropous substance appears to be composed of highly ive anisotropous rod-like bodies—sarcous elements, muscle-rods of a less refractive isotropous amorphous intermediate substance. nann distinctly denies that these elements are distinguishable muscular fibre while in a living condition; for those parts in these elements have made their appearance are non-irritable t exception.

some cases the anisotropous disks are the only parts of the lar divisions which have split into rods, the other parts not g any sign of a longitudinal differentiation; *e. g.* in muscular

fibres of insects which have died spontaneously or which have been treated with water, very diluted saline solution, or diluted alcohol. In most cases, however, especially in locustida amongst insects, and in vertebrate animals in general, the disintegration takes place through all the disks of the individual divisions; in this way the so-called primitive fibrils make their appearance. On observing the optical longitudinal section of a fresh muscular fibre for some time, the disks of the divisions, at first absolutely homogeneous, show immeasurably fine pale isotropous longitudinal lines; they are in almost regular distances from each other, not more than 0·001 of a millimètre. These lines gradually become brighter, and at the same time broader —their thickness exceeding the 0·0005th part of a millimètre—at the expense of those parts that lie between them, without the muscular fibre, as a whole, altering in diameter. Consequently it may be said, that the appearance of the longitudinal bright lines is caused, not by the swelling of a pre-existent intermediate substance, but by the shrinking, *i. e.* coagulation, of elements, which have been previously in close contact with each other; so that all the disks of the muscular division must be regarded as consisting in the living state of prismatic elements, which are so swollen that they touch each other completely, and which possess different chemical and physical properties in the different disks, but the same properties in the same disk. An intermediate fluid substance is not pre-existent, but is pressed out by those elements when they coagulate. In a second paper,* Englemann treats of the changes of the individual disks of the muscular division during contraction of the muscular fibres. For studying these, Englemann uses, like Flögel, perosmic acid. The living muscular fibre is dipped into a solution containing 0·5 to 2 per cent. of this reagent for a few seconds; it is then transferred into a ½ per cent. saline solution, which is afterwards replaced by alcohol in a slightly rising concentration (50 to 90 per cent.), and is finally placed in turpentine. The conclusions which Engelmann draws from his observations are briefly these:—

(*a*) The shortening force has its seat exclusively in the anisotropous layer; this latter thickens itself much more than the isotropous.

(*b*) The isotropous substance *decreases*, the anisotropous *increases* in volume during contraction; it must be therefore assumed that fluid which is expressed by the isotropous is imbibed by the anisotropous substance, *viz.* the latter swells, the former shrinks during contraction.

(*c*) The isotropous substance becomes darker, more opaque, the anisotropous brighter, more transparent, during contraction; the median disk, however, does not become brighter. From this it may be deduced that

(*d*) The isotropous substance becomes firmer, the anisotropous softer, during contraction.

Mr. Schäfer's views, having been already given in this Journal, need not now be repeated.

*Condition of the Blood in Yellow Fever.*—A long paper appears on this subject, by Professor Joseph Jones, M.D. (U.S.A.), in the 'New

* Ibid., vol. vii., parts 2 and 3.

York Medical Journal' for November last. The colour of the venous blood was purplish, between that of arterial and venous blood. When a drop of blood was allowed to fall upon a sheet of white bibulous paper, a central bright-red spot remained, with a surrounding bright areola of serum. The blood coagulated very slowly, and formed a large, loose coagulum, which contracted slowly and imperfectly. Thus in a one-thousand-grain specific gravity bottle, the coagulum filled the whole bottle, and from this amount of blood not more than 150 grains of golden-coloured serum could be collected at the end of forty-eight hours. The blood corpuscles tended to rapid dissolution in the serum, and, upon standing, the serum changed from this cause to a bright red. The reaction of the blood was carefully determined as it flowed from the vein, and found to be *alkaline*. I regarded this observation with interest, as, in several cases in which I had abstracted blood from the cavities of the heart, after death, it gave a decided acid reaction; but the present observation would seem to show that the acid reaction was due to *post-mortem* changes. Immediately after its abstraction, the blood was subjected to a rigid microscopical examination. Under a magnifying power of one-fifth of an inch (Smith and Beck, London), many of the blood corpuscles presented an irregular, stellated outline. When received under high magnifying powers, as the $\frac{1}{18}$th-inch immersion lens of G. and S. Merz, of Germany, with eye-glasses to magnify 1050 diameters, the crenated and stellated blood corpuscles were found to be studded upon the surface, all over, with nodular, rounded projections. The coloured blood corpuscles appeared to be undergoing changes of form, as if irregular transudations of the globulin were forming upon the surface. These changes were most marked and frequent upon the surface and outer portions of the clots, and resembled, in some respects, the amœboid movements of the colourless corpuscles; the nodules, however, were uniformly diffused over the surface of the corpuscles. When the blood was examined from the interior of the clot, the corpuscles were found conglomerated together, forming rolls or piles, adhering together by their flat surfaces, like the *rouleaux* of the blood of inflammatory diseases, and of the horse. The corpuscles which had been joined and agglutinated together by their flat surfaces, were normal in shape, and presented no stellated or nodulated outline, as was the case with the corpuscles from the surface of the clot, and from the surrounding golden-coloured serum. It appeared as if the exudation forming the nodules upon the free-coloured blood corpuscles had formed the band of cement between the opposing surfaces. Upon standing for twenty-four hours and longer, the coloured corpuscles tended to dissolve and lose their outline, and the serum became coloured from the escape of the colouring matter of the red globules. The coloured corpuscles appeared to be acted upon and altered by the urea and bile, which chemical analysis revealed in considerable amount in the serum. After standing in an open beaker glass, or in porcelain capsules, for forty-eight hours, numerous fibres made their appearance, as in other putrefying animal fluids, as blood and albuminous urine and serous exudations. But no living animalcule, or vegetable cells, or sporules,

or pigment granules were discovered, even after the most diligent search with high powers, ranging up to the $\frac{1}{20}$th of an inch objective, in the fresh blood. This paper is of considerable length and is worth reading.

*The Nervous System of Actinia.*—The following is an abstract of Dr. Martin Duncan's, F.R.S., paper in a late number of the 'Proceedings of the Royal Society.' After noticing the investigations of previous anatomists in the histology of the chromatophores, the work of Schneider and Rötteken on these supposed organs of special sense is examined and criticized. Agreeing with Rötteken in his description, some further information is given respecting the nature of the bacillary layer and the minute anatomy of the elongated cells called "cones" by that author. The position and nature of the pigment cells are pointed out, and also the peculiarities of the tissues they environ. It is shown that the large refractile cells, which, according to Rötteken, are situated between the bacilli and the cones, are not invariably in that position, but that bacilli, cones, and cells are often found separate. They are parts of the ectothelium, and when conjoined enable light to affect the nervous system more readily than when they are separate. Further information is given respecting the fusiform nerve-cells and small fibres noticed by Rötteken in the tissue beneath the cones; and the discovery of united ganglion like cells and a diffused plexiform arrangement of nerve is asserted. The probability of a continuous plexus round the *Actinia* and beneath each chromatophore is suggested, and the physiological action of the structures in relation to light is explained. The minute structure of the muscular fibres and their attached fibrous tissue in the base of *Actinia* are noticed; and the nervous system in that region is asserted to consist of a plexus beneath the endothelium, in which are fusiform cells and fibres like sympathetic nerve-fibrils. Moreover, between the muscular layers there is a continuation of this plexus, whose ultimate fibrils pass obliquely over the muscular fibres, and either dip between or are lost on them. The other parts of the *Actinia* are under the examination of the author, but their details are not sufficiently advanced for publication. The nervous system, so far as it is examined, consists of isolated fusiform cells with small ends (Rötteken), and of fusiform and spherical cells which communicate with each other and with a diffused plexus. The plexus at the base is areolar; and its ultimate fibres are swollen here and there, the whole being of a pale-grey colour.

## NOTES AND MEMORANDA.

**The Chair of Comparative Embryology at the College of France.**—The Academy, which has the right to nominate two candidates for this post, held a meeting for the purpose of nomination on the 9th of February. At this meeting the candidates to be recommended to the

Minister of Public Instruction for the chair of Comparative Embryogenesis at the College of France, were balloted for. The names of MM. Gerbe, Balbiani, and Dareste, were presented to the meeting, and the result of the voting was to select the two former gentlemen as the Academy's nominees for the post.

**An Inorganic Mimicry of Organic Life.**—Mr. J. Sidebotham, F.R.A.S., exhibited at a late meeting of the Microscopical Section of the Manchester Philosophical Society (January 19th) some curious specimens illustrative of this process. The title of the paper which he read upon the subject was "The similarity of certain Crystallized Substances to Vegetable Forms." In this he called attention to the formation of verdigris on insect pins, in old entomological collections. This substance makes its appearance where the pins pass through the thorax of the insects, and in length of time grows into a considerable mass of flocculent matter, of a brilliant green colour, and often breaks up the insects and also destroys the pins. It consists mainly of acetate or formiate of copper in combination with fatty or oily matter. On examination of various specimens under the microscope, they were found to present a great variety of forms, filamentous and ribbon-like structure, often resembling various fungi, in some cases so nearly, that it was difficult to believe that the fibres and fruit-like forms are not really organic bodies. Mr. Sidebotham expressed his opinion that these bodies were simply crystals, modified in their formation by the oil contained in the insects, with which the crystals are in some way combined. Some of the specimens exhibited were taken from insects collected twenty-five years ago.

## CORRESPONDENCE.

### Mr. Keith's Examination of Mr. Tolles' Objective.

*To the Editor of the 'Monthly Microscopical Journal.'*

Georgetown, D.C., *January* 20, 1874.

Sir,—My brief note to Dr. Woodward in connection with his trial of Mr. Tolles' objective, published in your Journal, does not seem to have conveyed fully my purpose in alluding to the additional lens. I wish to explain a little more fully my view of the matter.

All see that the limit 82° *depends* upon the *difference* of refractive power at a *plane surface;* that is, upon *two* variables; and, therefore, necessarily changes with a change of either of them.

If *balsam* is substituted for air between the objective and the cover, the refracting surface is practically removed from the cover to the posterior surface of the front lens, from a *plane* to a *curve*, and the limit, which *depends* upon the *curvature*, changes with *that* variable.

If *water* is substituted for air between the objective and the cover,

the *difference* of refractive power changes, and the limit, which also depends upon the *difference* of refractive power, changes with *that* variable. The only case in which the limit can remain the same is when the effect of change in the two variables is in opposite directions. In the case of an ordinary immersion objective the effect of the change in both variables is to increase the angle of total reflexion, whatever fluid is substituted for air between the surfaces.

The result is that 82° is no longer the *theoretical* limit of aperture under these circumstances, but a limit far greater, depending, of course, in some degree, upon the fluid used. For balsam it is 180° *theoretically;* what the opticians can do with it *practically* remains to be seen.

It seems to me that it is only from want of attention that Mr. Wenham has failed to take this very clear and easy abstract view of the matter.

Respectfully, &c., &c.

RENEL KEITH.

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## MR. STODDER'S LAST LETTER.

*To the Editor of the 'Monthly Microscopical Journal.'*

PADNAL HALL, ESSEX, *January* 31, 1874.

SIR,—Mr. Stodder's letter merits no reply further than to correct a misprint. For the thickness of cover referred to, instead of $\frac{1}{70}$th read $\frac{1}{75}$th of an inch. My authority is taken from page 97 of this Journal for Aug., 1873.

As I need not trouble my readers with remarks useless in the discussion of scientific truth, I leave Mr. Stodder in his appropriate garb of personalities. My argument has been preferably transferred to Colonel Woodward, from whom no such insinuations can come.*

Yours truly,

F. H. WENHAM.

* My explanations concerning this object-glass trial have brought invective from Mr. Stodder. In his eagerness to attribute dishonesty of intention to me, he overlooks the fact that the extraneous question of performance may be set at rest. No offer comes from him, but if he so desires, and will again send the object-glass by hands that he can trust, I shall be pleased to have it tested as an immersion, with any thickness of cover that he may choose, against my old dry $\frac{1}{8}$th as before, in the presence of a number of witnesses. It has not been considered proper amongst microscopists to hold public meetings, in order to pass judgment on the respective merits of object-glasses. In this I quite agree, but these insinuations induce me to make this offer, which can bring no opposition as the curves of this $\frac{1}{8}$th are now obsolete.

I am not a *trader* in object-glasses, but a teacher in their construction, and it is, I hope, with excusable pride that I see, as a consequence of my voluntary contributions (the series of which have recently been republished), amateurs not only here, but in *America also*, taking up this elegant manipulation with success. I am under no condition that binds me to reserve, and shall have further particulars to disclose.

## "Fair Play" on Dr. Pigott.

*To the Editor of the 'Monthly Microscopical Journal.'*

224, Regent Street, *Feb.* 5, 1874.

Sir,—As "Fair Play" has not ventured to appear, though directly challenged, I am led to infer that his relationship to Dr. Pigott is possibly too near for the legitimate assumption of the pseudonym chosen by him.

He asked for the name of "a single microscopist who has seen Dr. Pigott's experiments and will endorse Mr. Wenham's statements concerning them?" I do not remember the precise words, but our worthy President, in his anniversary address last night, laid emphatic stress on his opinion that, having seen the Aplanatic Searcher in Dr. Pigott's own hands, and having applied his mathematical skill to investigate the principle of its construction, he had come to the conclusion that its merits were mainly, if not wholly, fictitious: a verdict which the applause of the meeting seemed to cordially endorse. The President not only condemned the Aplanatic Searcher, but he even exposed the conceit of its pretended originality by reminding the meeting that Dr. Goring had used a similar instrument forty years ago, and afterwards discarded it.

I have no wish to write another word about the Aplanatic Searcher. With the greatest possible cheerfulness I say good-bye to it; but I should like to add just a line on the general question as to the value of Dr. Pigott's contributions to microscopy. I have gone through them with attention, and I am strongly impressed with the conclusion that there has been an excess of talk about his discoveries. Had he given us fewer defective diagrams, theorems, and formulæ, and, above all, less cause for suspecting his disingenuousness by frankly stating, *en passant*, from whence much of his text was transcribed;—then, I think, he would have better claims to that credit which "Fair Play" says "will be due to him when his views are finally established."

I remain, Sir, your obedient servant,

John Mayall, jun.

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## The Inventor of the Water-tight Caps.

*To the Editor of the 'Monthly Microscopical Journal.'*

289, Camberwell New Road, *Feb.* 17, 1874.

Dear Sir,—With regard to your notice in the last number of the 'Monthly Microscopical Journal' of the improvements in the water-tight caps brought out by me in 1872, and exhibited before the Society, I beg to inform you that I was not the inventor of them, but that they were modified from Mr. Stephenson's submersion microscope by a F.R.M.S., who gave the model to me to make what use of I might think fit.

Will you do me the favour of inserting this in the Journal?

I am, dear Sir, obediently yours,

E. Richards.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society—Anniversary.

King's College, *February* 4, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the meeting were voted to the donors.

The President having requested that two gentlemen might be elected as scrutineers, Mr. Gay was proposed by Mr. Curties, and seconded by Mr. Dobson; Mr. Suffolk was also proposed by Mr. Frank Crisp, and seconded by Mr. Lealand; and these gentlemen having been elected by the meeting, the ballot for Officers and Council for the ensuing year was taken in the usual way. The scrutineers subsequently reported that no alterations had been made in any of the balloting papers, and the gentlemen whose names had been printed on the "house-list" were declared to be duly elected.

The Annual Report of the Treasurer was then read by the Secretary, and having been put to the meeting was received and adopted unanimously.

The Secretary also read the business portion of the Annual Report of the Council.

The President said that the Annual Meeting being the occasion for making any alterations or additions to the bye-laws of the Society, it would be proper to bring before them a new rule which it was proposed to add to the others. The rule would read as follows:—

"Any person residing out of the United Kingdom cultivating microscopical science, and desirous of corresponding with the Society, may be proposed and elected as a Corresponding Fellow, subject to the condition of recommendation by the Council with respect to Honorary Fellows by clause 15.

"Corresponding Fellows so elected shall not be chargeable with any entrance fee or annual subscription, so long as they continue to reside out of the kingdom."

It was known that there were many persons who resided abroad and were pursuing microscopical studies, who, though not of sufficient distinction to be elected Honorary Fellows, might nevertheless be of much use to the Society by being connected with it, and it had seemed desirable that there should be an opportunity of having them so connected as Corresponding Members—the position of Honorary Fellow being still reserved for distinguished persons. The new rule had therefore been framed to give the Society the power of associating with itself those persons from whom valuable information might be obtained, and it was believed that the arrangement would prove of advantage to microscopical science.

The proposed new bye-law was then put to the meeting and carried unanimously.

The Secretary said that, in consequence of this addition to their rules, a verbal alteration would become necessary in clause 12 (which explained the position of Honorary Fellows) in order to make it suit with the new rule—

To clause 12 after *Honorary* add "and Corresponding."

The clause having been altered as proposed, was put to the meeting and confirmed.

The President then delivered the Annual Address, which will be found printed at p. 89.

It was moved by Dr. Braithwaite, and seconded by Mr. Wenham, "that the cordial thanks of the Society be presented to the President for his Address, and that it should be printed and circulated in the usual way.

Mr. Slack then put the motion to the meeting, and it was carried unanimously by acclamation.

The President having expressed his acknowledgment of the vote of thanks, and there being no response to his inquiry for suggestions from any Fellow present as to the working of the Society, the meeting was adjourned to March 4th.

LIST OF OFFICERS AND COUNCIL OF THE ROYAL MICROSCOPICAL SOCIETY, ELECTED FEBRUARY 4TH, 1874.

*President.*—Charles Brooke, M.A., F.R.S.

*Vice-Presidents.*—Robert Braithwaite, M.D., F.L.S.; John Millar, L.R.C.P., F.L.S.; William Kitchen Parker, F.R.S.; Francis H. Wenham, C.E.

*Treasurer.*—John Ware Stephenson, F.R.A.S.

*Secretaries.*—Henry J. Slack, F.G.S.; Charles Stewart, M.R.C.S., F.L.S.

*Council.*—James Bell, F.C.S.; Frank Crisp, LL.B., B.A.; William J. Gray, M.D.; John E. Ingpen, Esq.; Samuel J. McIntire, Esq.; Henry Lee, F.L.S.; William T. Loy, Esq.; Henry Lawson, M.D.; Henry Perigal, F.R.A.S.; Alfred Sanders, M.R.C.S.; Charles Tyler, F.L.S.; Thomas C. White, M.R.C.S.

## *Annual Report of the Royal Microscopical Society.*

The Secretaries report that the Society's collection of books, instruments, and objects are in good condition; few purchases have been made, but some valuable additions have been received, the most important of which are a Smith and Beck Binocular Microscope, with powers and apparatus, from Charles Woodward, Esq.; 42 Type Slides of North American Algæ, from Dr. Wood and Mrs. Quimby; 3 Slides of Diatoms, from Mr. Kitton; and 4 from Captain John Perry.

The Society has been well supplied with papers of interest and importance during the past year, extending over a considerable range of topics. Dr. Urban Pritchard contributed a paper "On the Structure and Functions of the Rods of the Cochlea." Mr. Kitchen Parker brought under the Society's notice the Morphological Results arrived at by studying the Development of the Face of the Sturgeon; and

Dr. Schmidt, "The Origin and Development of the Coloured Blood Corpuscles in Man."

Mr. H. Davis brought under our notice a New Callidina, and recounted a series of experiments on the Desiccation of Rotifers. Dr. Maddox called attention to an Organism found in Fresh-pond Water; to an Entozoon with Ova found encysted in the Muscles of a Sheep; and also to a Minute Plant found in an Incrustation of Carbonate of Lime. Dr. Braithwaite continued his valuable papers on the Bog Mosses. Mr. Parfitt described and figured a peculiar microscopic animal, which he named "*Agchisteus plumosus*." As bearing on the controversies relating to the origin and development of Infusorial Forms, particular attention may be called to the valuable papers of the Rev. Mr. Dallinger and Dr. Drysdale, the first of which was entitled "Researches on the Life History of a Cercomonad: a Lesson in Biogenesis." The care with which the observations and experiments detailed by these gentlemen were made; and the high powers, up to a Powell and Lealand $\frac{1}{50}$th, successfully employed, mark them out as deserving special study; they carry our knowledge of what appear to be true examples of sexual actions and development down to objects more minute than any in which they were previously observed. They trace a variety of forms to the same species, and acquaint us with ova or germinal particles so minute as to evade explicit definition with any amplification that could be employed. Mr. Wenham has contributed a method of Dissecting Podura Scales, which leads him to reiterate his denial of the much-disputed "beads." Mr. Kitton has described some new species of Diatoms, while fresh interest has been excited in the investigation of the structure of some of these organisms by Mr. Stephenson's "Observations on the Optical Appearances presented by the Inner and Outer Layers of Coscinodiscus when examined in Bisulphide of Carbon and in Air," a paper which was illustrated by careful drawings made by Mr. Charles Stewart. Mr. Dallinger described and sent for exhibition specimens of Lecture-Illustrations prepared on Glass by a new method.

### Books Purchased during the past Year.

Annals of Natural History. 2 Vols.
Quarterly Journal of Microscopical Science. Vol. XXI.
Plowright's Sphæriacei. Part 1.
Monograph of the Collembola and Thysanura. By Sir John Lubbock.
Monograph of the British Annelids. Part 1. By Dr. McIntosh.
Lindley's Vegetable Kingdom.

### Books Presented.

A Contribution to the History of Fresh-water Algæ of North America. By H. C. Wood, jun.
On some Remarkable Forms of Animal Life from the Great Deeps off the Coast of Norway. By G. O. Sars.
Lichen Flora of Great Britain. By Rev. W. A. Leighton.
Transactions of the Linnean Society.
Several pamphlets and papers, as well as the journals of other societies in exchange for our own, have been periodically announced in the 'Monthly Microscopical Journal.'

APPARATUS AND SLIDES.

| | |
|---|---|
| A Smith and Beck Binocular Microscope and quantity of Apparatus, &c. .. .. .. .. .. .. .. .. .. | *Chas. Woodward, Esq.* |
| 3 Slides .. .. .. .. .. .. .. .. .. .. .. | *Mr. Kitton.* |
| 42 Slides of North American Algæ .. .. .. .. .. | *Dr. Wood and Mrs. Quimby.* |
| 4 Slides .. .. .. .. .. .. .. .. .. .. .. | *Capt. Perry.* |

Nine Fellows have been elected during the past year.
Five Fellows have deceased during the same period.

Charles Palmer Gibson, of Hull, elected Feb. 7, 1872, died June 9, 1873.
*William Richard Morris, of Deptford, elected March 11, 1851, died Jan. 11, 1874.
*John Augustus Tulk, of Addlestone, Surrey, elected Jan. 11, 1860, died Dec. 17, 1873.
**Cornelius Varley, of 337, Kentish Town, elected Jan. 29, 1840, died Oct. 2, 1873.
John Martin, M.D., of Portsmouth, elected Dec. 11, 1867, died ——.

The obituary of this Society during the past year comprises the name of one of the veterans of microscopic science, and of the founders of the Society, Cornelius Varley. In his younger days he was an artist of some reputation, and with his brother, John Varley, the celebrated water-colour painter, was one of the founders of the Society of the Painters in Water Colours, and of these he was the survivor; but what is of more immediate interest on the present occasion is his career as a microscopist.

He was born in 1781, and having lost his father, his uncle, Samuel Varley, took charge of him in 1793, and was assisted by him in his scientific pursuits, and in the manufacture of physical apparatus for optical, electrical, and other experiments.

In 1794 he effected a great improvement in the art of polishing lenses, by substituting for the silk and cloth then in use beeswax hardened with "crocus martis," or oxide of iron; this compound is, it is believed, still found to be the best for the purpose, and is still in use by most manufacturers. For very small lenses he used shellac hardened with polishing oxides.

In 1795, at a meeting of a "Friendly Microscopical Society," founded by his uncle, the lenses made by C. Varley were declared to be the best then exhibited; and about this date he manufactured many small and perfect lenses of 0·016 inch focus, and three lenses of only 0·01 inch focus. In 1801 he patented his "Graphic Telescope," and subsequently invented his "Graphic Microscope."

From 1801 until 1823 C. Varley chiefly occupied his time in the pursuit of the fine arts—his favourite study—and to which he occasionally devoted himself during the remainder of his life, having commenced and completed his last picture in his ninetieth year: he made great use of the microscope in the selection of suitable pigments, and by the same means discovered the causes of decay in water-colour drawings. By the aid of the microscope he superseded the monopoly held by the Belgians in lithographic stones; having access to an extensive geological collection, he sought for and met with specimens which, under a high power, presented a texture similar to that of the

Belgian stones. These specimens were from North Wales, and for a while were in use in this country.

In 1824 he invented his first lever stage-movement for the microscope for following the movements of live objects, for which he received the large silver medal of the Society of Arts. This was subsequently considerably improved in effecting the object in view by means of a single lever, and for the latter he received the gold Isis medal of that society; and it was considered to be the best lever movement of the day.

In 1826 he produced a plano-convex diamond lens, which possesses one advantage over lenses of glass in having about double the magnifying power with the same radius. This lens was exhibited at the Royal Institution in February, 1826, on the occasion of the third Friday Evening Lecture. But the diamond labours under the disadvantage of having no medium of higher refrangibility, by means of which its aberrations may be corrected; and the construction of diamond lenses was not pursued further, as the doublets of Wollaston, and subsequently achromatic and aplanatic combinations, altogether superseded the use of single lenses in the construction of compound microscopes. It may here be remarked that the diamond, as belonging to the regular system of crystals, and therefore possessing no double refraction, is the only gem that stood any chance of useful application; other gems having high refractive indices belong to crystalline systems in which double refraction is invariably present, a property which must obviously interfere with optical definition.

C. Varley was one of those microscopists who met and founded this Society on September 3, 1839. In the second volume of the Society's Transactions will be found an elaborate description of the growth and development of the "*Chara vulgaris*." One characteristic of this plant discovered by him was the emission of certain ciliated bodies (to which he gives no name, but which are now known as Antherozoids), which possess spontaneous motion for about two hours, and closely resemble the spermatozoa of animals. Various other papers on this and kindred subjects will be found in the Journal of this Society, and in that of the Society of Arts, relating to the structure of this plant, and to that of the *Nitella flexilis*, *N. translucens*, and *N. hyalina*. The plates in these papers were accurately drawn from the living plants by his "Graphic Microscope." He also contributed papers on the fungoid disease of the common house-fly, of the nature of which he was the discoverer.

Mr. Varley preserved his intellect to the last, and was professionally occupied three weeks before his decease, which occurred on October 2, 1873, and consequently in his ninety-second year, after having been in a state of *coma* for twelve hours.

John Augustus Tulk, Esq., of Addlestone, Surrey, was elected in January, 1860, and died in December, 1873, aged fifty-nine. Although unable from the locality of his residence to attend the meetings of the Society frequently, he was much interested in microscopical research, having in 1857 become acquainted in Edinburgh with the late Dr. Greville, one of the most painstaking and successful investi-

gators of the Diatomaceæ; to this special subject he almost exclusively devoted his attention, and wrote a paper on collecting, cleaning, and preparing Diatoms, which appeared, in 1863, in the third volume of the 'Microscopical Journal,' New Series.

Mr. Richard Morris, M.S.C.E., was elected in March, 1851, and died in January, 1874, aged sixty-five. He was engineer of the Kent Waterworks Company, and at one period of his life took a great interest in the meetings of the Society. His end was a melancholy one; it appears, from the evidence on the coroner's inquest, that he must have dropped dead from serous apoplexy on his way home on foot from the works.

Mr. Charles Palmer Gibson, of Hull, was elected only in February, 1872, and died in June, 1873, at the early age of thirty-two.

JOHN WARE STEPHENSON IN ACCOUNT WITH THE ROYAL MICROSCOPICAL SOCIETY.

*Dr.* *Cr.*

| 1873. | £ | s. | d. | 1873. | £ | s. | d. |
|---|---|---|---|---|---|---|---|
| To Balance brought from 31st Dec. 1872 .. .. | 136 | 0 | 10 | By Cash paid for Journal .. | 239 | 11 | 6 |
| „ Half-year's Dividend on 1082*l.* 0*s.* 11*d.* Consols | 15 | 19 | 3 | „ Rent and Attendance at King's College .. .. | 57 | 19 | 3 |
| „ Ditto on ditto .. .. | 16 | 0 | 7 | „ Reporter .. .. .. .. | 9 | 9 | 0 |
| „ Commuted Subscription | 18 | 18 | 0 | „ Mr. Reeves' Salary .. | 85 | 5 | 0 |
| „ Annual Subscriptions, &c. | 440 | 3 | 0 | „ Ditto for Commission .. | 11 | 10 | 0 |
| „ Screw-tools sold .. .. | 0 | 7 | 0 | „ Ray Society for 1873 .. | 1 | 1 | 0 |
| „ Journals sold .. .. .. | 0 | 3 | 0 | „ Fire Insurance .. .. | 1 | 4 | 0 |
| | | | | „ Stationery and Printing | 33 | 14 | 2 |
| | | | | „ Petty Cash .. .. .. | 42 | 0 | 0 |
| | | | | „ Balance remaining 31st Dec. 1873 .. .. .. | 145 | 17 | 9 |
| | £627 | 11 | 8 | | £627 | 11 | 8 |

Jan. 28, 1874. Examined and found correct,

W. T. SUFFOLK.
E. W. JONES.

Donations to the Library and Cabinet since Jan. 7, 1874:—

| | From |
|---|---|
| Nature. Weekly .. .. .. .. .. .. .. .. .. .. .. | *The Editor.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts Journal. Weekly .. .. .. .. .. .. | *Society.* |
| Quekett Journal, No. 25 .. .. .. .. .. .. .. .. | *Ditto.* |
| Annual Report, &c., of the Brighton and Sussex Natural History Society, 1873 .. .. .. .. .. .. .. .. | *Ditto.* |
| Microscopic Examination of Air. By Dr. Douglas Cunningham | *Author.* |
| One Slide .. .. .. .. .. .. .. .. .. .. .. .. | *Capt. John Perry.* |

John Shalders Crisp, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
*Assistant Secretary.*

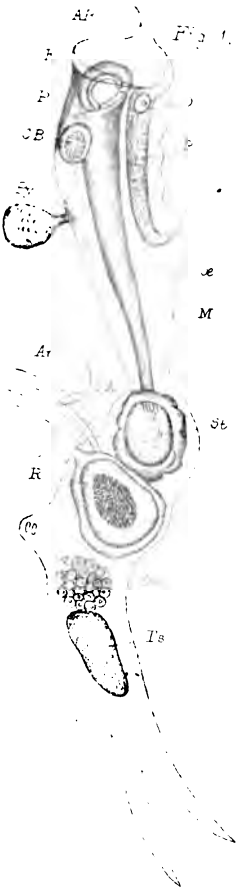



Fig. 1.

Fig. 1a.

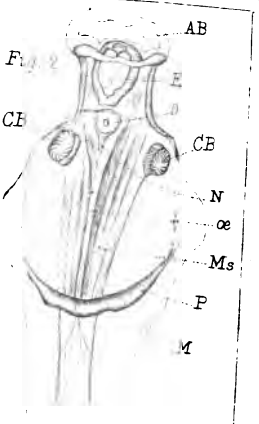



Fig. 2.

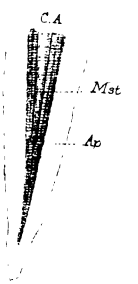



Fig. 4a.

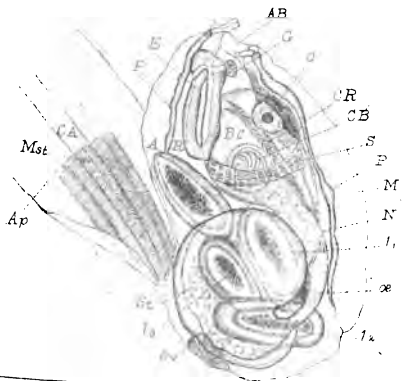



Fig. 4.

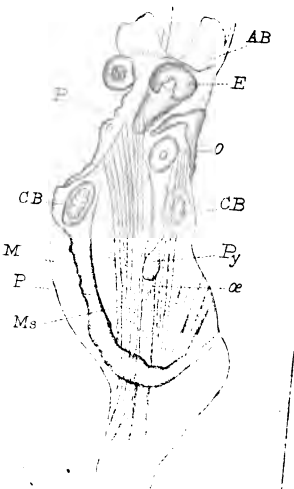



Fig. 3

A. Sanders delt.

W. West & Co. imp.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1874.

## I.—*Contributions towards a Knowledge of the Appendicularia.*

By ALFRED SANDERS, F.L.S., F.R.M.S., Lecturer on Comparative Anatomy at the London Hospital Medical College.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *March* 4, 1874.)

PLATE LVI.

THE interest which formerly attached to the Appendicularia, on account of their resemblance to the larvæ of the Ascidians, was increased tenfold when Kowalewsky pointed out the relation of the latter to the vertebrata through their mode of development. It was with no little pleasure, therefore, that one fine breezy morning, during the spring-tides of the month of September, I recognized one of these forms in a vessel of sea-water from the harbour of Torquay, which I was examining. They were not very plentiful, perhaps there might have been half-a-dozen or so in each bucket of water. Neither did they stay very long; shortly after high water no more were to be found, and it was necessary to wait until next day for further specimens; and at the end of three days they returned no more, and the supply was inexorably cut off.

The animal combined the characters of two distinct species,

EXPLANATION OF PLATE LVI.

FIG. 1.—Side view of the long-bodied form of Appendicularia.
„ 1*a*.—Distal termination of the appendage.
„ 2.—Dorsal view of the anterior part of same.
„ 3.—The same, seen in a position intermediate between the dorsum and the side.
„ 4.—Side view of the short-bodied form.
„ 4*a*.—Distal termination of the appendage.

Letters same in all.

A, Anus.
A, B, Inhalent aperture of the branchial chamber.
Ap, Appendage.
B, C, Branchial chamber.
C, B, Ciliated branchial apertures.
C, A, Central axis.
C, R, Ciliated ridge.
E, Endostyle.
G, Granular body.
I, 1; I, 2; I, 3; Intestine: the greater part seen through the walls of the stomach.
M, Mantle.
M, S, Smooth muscular fibre.
M, St, Striated do. do.
N, Nerve.
O, Otolithe.
Œ, Œsophagus.
Ov, Ovary.
Py, Pyriform body.
R, Rectum.
S, Stigmata.
St, Stomach.
Ts, Testis.

described by Gegenbaur,* at Messina, as regards the appendage, but differed from either in the shape of the body. One of these species was named by that author *A. acrocerca;* this had a tail which was inserted by a thin, short stalk, and terminated in a fine-pointed free extremity. The other he termed *A. furcata.* In this the tail was inserted by a broad basis, and ended in a forked extremity; this species also further resembled the subject of the present memoir in having the posterior end of the body also forked.

The external form of the species I am about to describe is more elongated, and not so compact as that of those animals described by Gegenbaur. From a wide anterior extremity a comparatively narrow neck leads down to a posterior enlargement, which contains such viscera as the creature possesses, and terminates behind in two projecting points. The whole presents a gentle double curve, being convex towards the dorsum at the anterior and posterior end, and towards the ventral surface in the middle. The anterior section of the body is strengthened by thick granular walls, which at the entrance into the pharynx expand into two thick lips, which, being continued by diaphanous material round the circumference, give an appearance somewhat resembling the preoral disk of a rotifer, as it might appear if the cilia were suppressed. The remainder of the body is enclosed in a transparent glassy case of such extreme delicacy that only one specimen out of the number that I examined retained its outline for a sufficient length of time to be sketched, all the others became crumpled up more or less in a few minutes.

In some cases instead of the anterior extremity terminating in a smooth disk it is prolonged into one or two sharply-pointed curved horns, Fig. 1; in others it presents several projecting spikes and prominent ridges, Fig. 3; in fact, the shape of this part appeared to vary in every specimen examined, as also did the proportions and position of the thick granular walls. The tentacula described by Dr. Moss † were not present in these specimens.

The pharynx commences as a wide tube behind the preoral disk, and gradually tapering as it passes through the attenuated part of the body it enters a globular thick-walled chamber, which appears to be the stomach; there is no appearance of a branchial chamber in this animal; but the whole of the pharyngeal tube is ciliated; the cilia are more apparent at the posterior end. At the point where the pharynx enters the stomach, a bundle of very long cilia project into the cavity of the latter with a flickering motion; the rest of its surface is covered by short cilia. Behind, the stomach opens into the side of a larger thick-walled chamber, having its longer axis placed obliquely across the body; this is the sole representative of the intestine, and opens immediately on the

* Zeit. f. W. Zool., Bd. 5 and 6, 1854–56.
† 'Trans. Linn. Soc.,' vol. xxvii., 1870.

outer surface at its antero-dorsal extremity. It contains a mass of dark yellow granules, which is kept continually revolving by means of the strong cilia which line its internal surface; the walls of both these chambers are thick, and appear to be glandular, especially those of the stomach. A peculiarity which distinguishes this appendicularia from all others is, that the anus opens behind instead of in front of the attachment of the appendage.

A pair of ciliated branchial openings exist near the anterior end of the body, which are situated one on each side of the wider part of the pharynx; these openings were described by Gegenbaur * as leading into a series of internal channels, but Professor Huxley † demonstrated that they opened directly into the branchial chamber; in the present subject they appear to be simple ciliated pits, and I could make out no communication between them and the pharynx (for there really is no branchial chamber). Unfortunately I omitted to feed the animals with indigo; perhaps if I had done so the connection between the two might have become more apparent; the cilia of the structures in question are very large, and are so curved that all the points meet in the centre.

The endostyle which Professor Huxley ‡ considered to be the optical expression of a fold of the branchial chamber, here appears to be rather a complicated structure; on a dorsal view (Fig. 2) it is seen to be of an oval figure with the posterior end truncate; its external walls are thick, and internally it is occupied by what seems to be a hollow cavity, which is divided into two by a longitudinal partition, while anteriorly and posteriorly two small spaces are left, the latter of which is truncate like the external wall; on a side view (Fig. 1) the shape is totally different, it now appears to have a crescentic form, with external thick granular walls lined internally by a thin layer of a bright substance; again, in another specimen, when seen in a position between those two, it looks like a conical body capped anteriorly by a kidney-shaped mass. Whether this diversity of appearance is due to a change of position simply, or to individual variations in different subjects, I am not prepared to say, but the probability is that it is due to the latter cause, considering the modifications to which the external tunic is subject.

A very distinct vesicle containing an otolithe is present imbedded in the thickened ventral wall of the anterior part of the body; this is surrounded by a granular nervous mass from which a nerve could be traced running along the pharynx towards its posterior extremity, but I failed to discover in this species the extensive nervous ramifications described by Dr. Moss, § neither was the ganglionic chain running along the appendage apparent.

In many specimens a curious pyriform body was found attached

* *Loc. cit.*
† 'Quar. Jour. Mic. Sci.,' vol. iv., 1856.
‡ *Loc. cit.*
§ *Loc. cit.*

to the dorsal surface of the anterior part of the mantle; in several cases it was met with lying detached by the side of the animal, having been broken off by the covering glass; in one case (Fig. 2) it looked as if it were situated within the pharynx, it having been twisted round, and so placed as to be seen through the parietes of the body.

In several cases bundles of smooth muscular fibres were visible which seemed to run from the walls of the anterior part of the body to be inserted into the commencement of the neck.

A spherical body resembling a mulberry in shape is situated at the posterior end of the animal behind the intestine; it appeared to be composed of rather large cells, each containing a nucleus; this could be nothing else than the ovary. Gegenbaur saw this body, and mentions that in one specimen it contained a large ovum in the centre, but in my specimen no other contents than the above-mentioned cells were visible. In many specimens there was an elongated granular body in close juxtaposition to the posterior side of this ovary. I had no opportunity of observing into what structures these granules were developed, but Gegenbaur has described in *A. furcata* and *A. acrocerca* an organ occupying the same position, which he says contained zoosperms at one period of its existence.

A pair of curious bodies are situated in an elevation of the diaphanous mantle behind the anus on each side of the body; they seem to be composed of carbonate of lime, but their use is by no means apparent. In some specimens they resemble an acorn in shape, in others they are simply oval.

With regard to the organs of circulation, Gegenbaur mentions that the heart is the easiest of all the organs to be seen; it appears strange, therefore, that this viscus should have entirely escaped me. The only appearance which could be interpreted as having anything to do with that organ occurred unfortunately in the last specimen that came through my hands, so that I was unable to come to any definite conclusion about it. The appearance consisted of several curved rods, which seemed to embrace about half the circumference of the stomach, and to pulsate with great rapidity and regularity in a fore-and-aft direction. These curved rods might well have been the optical expression of folds in the transparent walls of the organ in question, and as the specimens I am describing undoubtedly belong to a species distinct from either of those discovered by Gegenbaur, it might happen that the heart would occupy a different position.

The appendage differs from that structure in both *A. furcata* and *A. acrocerca* in shape and mode of attachment to the body. At the free extremity it is bifid, as in the former species; at the proximal end it has the same form, as in the latter species. The central axis forms the only means whereby it is united to the body,

and it seems to be inserted into the front wall of the intestine; it is surrounded by the usual layer of striated muscular fibres, which do not quite extend to the proximal end of the central axis. Neither the vascular canal described by Dr. Moss, nor the ganglionic chain first mentioned by Professor Huxley, is visible in this specimen, but the individuals were so few in number, and the duration of their stay was so short, that the whole of my attention was concentrated on the body, so that those structures, had they existed, might have perhaps escaped notice. The quadrangular elevations on the appendage which were mentioned by Gegenbaur * are not present in this specimen.

The movements of this animal when under examination are so constant and so vehement, that it is impossible to use the camera lucida in drawing the outlines of its body; the proportions, therefore, of its different parts and their relative position to each other may not be quite exact; but I have endeavoured to get as near an approximation to the truth as is possible by the unaided eye. The length of the specimen given in Fig. 1 is about 0·56 mm., while the appendage extends to about three times that length.

A few days after having found the above-described species, I came across a supply of the short-bodied division of the Appendiculariæ at Weymouth. These † presented several points of interest, and differed a good deal in detail from the typical *A. flabellum*. They were rather more quiet under the microscope, so that it was possible to get a tolerably good outline with the camera lucida. They present a pitcher-like form, and the outer tunic is composed of an extremely transparent material, but the viscera, with the exception of the posterior end, are enclosed in a covering of a granular nature. At the anterior extremity this granular wall is continuous with a fine double membrane, which turns inwards round the inhalent aperture, and becomes continuous with the walls of the branchial chamber, which is a globular cavity occupying the anterior portion of the body. Anteriorly, this chamber opens externally by a circular aperture, while posteriorly, it tapers off gradually into the œsophagus. At about the centre it is encircled by a band consisting of three rows of what I take to be square openings or stigmata. If this interpretation should turn out to be correct, these stigmata form a remarkable approximation to the ordinary forms of Ascidians. In no accounts of these animals that I have been able to obtain can I find any mention of such structures.

I could only find one ciliated branchial aperture, and that was on the side of the body opposite to that which was turned towards the eye, that is to say, if the appendage be considered as being

* *Loc. cit.*

† Their length varied from 0·33 mm. to 0·60 mm., that of the tail from 1·08 mm. to 1·60 mm.

attached to the dorsal surface, and the nervous system as lying on the ventral side of the body, this single branchial aperture would be on the left side. I did not see any streams of water, but if the quadrangular figures really represent stigmata, then it is to be supposed that the water would pass through these to enter the cavity of the body, and then make its exit through the branchial aperture. This is evidently quite a different arrangement to that in *A. flabellum*, in which species, as described by Professor Huxley,* the two branchial apertures communicate directly with the branchial chamber. The branchial chamber is traversed by a ridge which runs obliquely across from about the middle of the endostyle to the commencement of the œsophagus; this ridge is provided with longer cilia than the remainder of the cavity. I did not see that it divided into two branches at the anterior part, as mentioned by Professor Huxley in *A. flabellum*.†

The endostyle is very thick; on a side view it presents a fissure down the centre, which makes it have the appearance of a double-fanged tooth; it extends backward nearly as far as the rectum.

The œsophagus commences as a gradual tapering of the branchial chamber; it passes backward towards the posterior end of the body, and there turning slightly towards the dorsal surface it enters the stomach. It is ciliated throughout. The ciliated ridge enters its anterior extremity; in fact, there is no line of demarcation between it and the branchial chamber.

The stomach has the form of a circular disk, which occupies nearly the posterior half of the right side of the body; at its postero-ventral side there is a deep notch, into one side of which the œsophagus enters. Its postero-dorsal walls are thicker than the antero-ventral. The convexity on this latter aspect fits into a corresponding concavity formed by the branchial chamber and the œsophagus. That part of the stomach which has thicker walls appears to be glandular, and is provided with numerous hemispherical papillæ.

The intestine has the appearance of four oval chambers with thick walls; when the contents are not passing from one to the other they seem to be perfectly distinct, and no apertures of communication are visible; I will call these chambers Nos. 1, 2, and 3. Commencing from the stomach, No. 1 is situated on the left side of the body even with the antero-ventral wall of the stomach, with which it communicated by an aperture coextensive with its length; in fact it appeared to be nothing more than a pouch of the stomach directed towards the left side, and evidently corresponds to the left lobe of the stomach in *A. flabellum*; it would not have been here classed with the intestine had it not been for the circumstance that the fæces first begin to be formed therein. No. 1 opens by its posterior extremity into chamber No. 2, which is of the same

* *Loc. cit.* † 'Phil. Trans,' 1851.

shape and of about the same size; this is placed transversely across the posterior end of the body; the aperture of communication between the two is very distensible, although not at all apparent when nothing is passing. The fæces are made up into oval masses by a material of some considerable tenacity, and I have watched these masses pass from No. 1 to No. 2, and *vice versâ*, for some time without their breaking or becoming mixed up with each other; as they passed lengthwise the opening at last became so large that the cavities of the two chambers seemed thrown into one. I concluded that these masses moved backward and forward because No. 3 being full they could not get any farther.

The third chamber emerges from the anterior wall of the dorsal end of No. 2, and passes forward parallel to and in contact with No. 1; it is of an oval form, and appears to be provided with the same kind of parietes as the other two. I have not actually observed the passage of fæces between No. 2 and No. 3, but on one occasion I happened to observe that No. 2 was full while No. 3 was empty. A short time after, on again looking at the animal, the case was reversed, for then No. 3 had become full and No. 2 was empty.

The fourth compartment might be denominated the rectum; it is continued in the same direction as No. 3, but in a line slightly moved towards the anterior extremity; it communicates with No. 3 at a point which is rather on the ventral side of its anterior end. This chamber also is oval, has thick walls, and terminates in the anus, which is a small papilla situated just in front of the attachment of the appendage. As in Gegenbaur's specimen, the anus appears to open between the wall of the body and the mantle, but that there is an opening through the latter, although not visible, is certain, for the fæces were seen to pass through the anus and the mantle into the surrounding medium. The intestine in these specimens is seen to be more complicated than in the species described by Professor Huxley and Professor Gegenbaur, as their figures only show a simple nearly straight tube running between the stomach and the anus.

With regard to the circulatory system, I could find no indication of a heart. I cannot imagine if it had been present in these animals that it would have escaped notice, although according to Professor Huxley it is very difficult to be seen in *A. flabellum*, in which species it is situated between the two lobes of the stomach and the insertion of the appendage, a space which in my examples is occupied by the intestine.

The otolithe is a very conspicuous object occupying the centre of a transparent vesicle on the ventral side of the body; this is surrounded by the granular nervous ganglion, from the posterior end of which a nerve runs downward over the right side of the œsophagus until it reaches the stomach, where it becomes lost to view; anteriorly the ganglion is prolonged towards the anterior extremity

and from this part a line is given off to surround the branchial chamber, which subsequent information has induced me to think must be part of the œsophageal nervous ring. The ciliated sac mentioned by Professor Huxley and Dr. Moss was not visible in this species.

Only one granular spherical body is present near the endostyle, instead of two, as is generally described.

It is to be presumed that these specimens were quite young, for the only structure which could by any possibility be referred to the generative organs was a small granular mass attached to the posterior wall of the body immediately behind the stomach; this most probably would be developed into a testis, as it occupies the position ascribed to that organ by Professor Huxley, in *A. flabellum*.

The appendage is attached to the body by a broad base, the central axis is distinctly articulated to the tunic, close to the dorsal border of the stomach, so that the idea entertained by Professor Gegenbaur that this was a hollow vessel communicating with the fluids of the body is untenable; the central axis terminates distally in a point at a short distance from the end of the tail; two fibres of striated muscular fibres are situated in front of and two behind the rod, and one on each side; they gradually taper to a point, and, extending beyond the end of the axis, terminate close to the distal extremity of the appendage; they are striated as far as their termination. The rest of the appendage is formed of an expansion of a diaphanous material, resembling that which constitutes the tunic of the animal; the posterior border was always found turned down at the proximal end. After being some time under examination this part of the appendage develops irregular, longitudinal, and transverse markings, which, however, do not appear to me to be anything more than a crumpling of the external membrane, and nothing resembling a layer of epithelium was to be seen.

Since reading this paper I have had an opportunity, through the kindness of Mr. Stewart, of consulting an extremely interesting monograph by Dr. Hermann Fol.* The author divides this family into three genera, *viz.* Oikopleura, Frittillaria, and Kowalewskaia. The long-bodied form that I have described belongs to the genus Frittillaria, in which Dr. Fol makes five species; from all these my specimens differ in the shape of the body and tail, in possessing smooth muscular fibres, in the direct communication of the stomach with the rectum, in the presence of a pyriform body, and in being much smaller.

The short-bodied form belongs to Dr. Fol's genus Oikopleura, of which he likewise describes five species; from all of which my specimens also differ in the shape of the stomach, intestines, and

* 'Études sur les Appendiculaires,' reprinted from the 'Mém. de la Société de Phys. et d'Histoire Nat. de Genève,' tom. xxi., 2me partie.

tail, in the presence of a band of stigmata, in the occurrence of only one branchial aperture, and in being smaller. One curious point the author makes out, is that the transparent outer tunic is the commencement of the formation of the "Haus" described by Mertens.* I had not the good fortune to meet with this curious structure in a fully-developed form, but the constancy of the occurrence of the transparent tunic makes me doubt this interpretation as applied to the present species.

* 'Mém. Acad. St. Pétersbourg,' 6$^{me}$ série, tom. i., 1831.

## II.—*Note on the Verification of Structure by the Movements of Compressed Fluids.*

By Dr. Royston-Pigott, F.R.S., &c.

(*Read before the* Royal Microscopical Society, *March* 4, 1874.)

In examining scales, one is often struck by the appearance of the obliteration of structure. In many cases the structure can be made to reappear by regulated pressure, especially in beaded scales.

In using Powell and Lealand's dry $\frac{1}{8}$th, of their new and best construction, the covering glass is often found too thick for nice correction; but in some cases it is just thick enough to permit contact between the front of the objective and the covering glass. In such cases a slight alteration of *the focus* by means of the screw-collar alone administers a very delicate increase of pressure.

In this way I have frequently observed the obliterated portion change its form, and this demonstrated the presence of oil natural to the scale, which, under delicate manipulation, may be made to shrink up into a globule, revealing the structure anew, notwithstanding apparent obliteration.

In these experiments one thing has struck me forcibly—*viz.* the disappearance and reappearance of minute details.

In water-lenses, a cracked cover—unfortunately a no uncommon catastrophe—becomes often a most instructive study. For the last five years I have been greatly interested at seeing the sudden discharge of water along grooves, channels, and beads. But of all the liquids used for this purpose of developing structure gradually out of obliteration, I have found none equal to *Rangoon oil.*

By this agency of oil and delicately-graduated pressure, by which the covering glass may be made to approach the slide less than the fifty-thousandth of an inch, one, two, three, then four, then a row, and finally a large surface of beading may be made to peep as it were out of the uniform-looking blank of the oily obliteration. And I would here state, *en passant*, that Rangoon oil seems to have a greater obliterating power than water.

It is well known now, that when the refractive index of a fluid and of substance are alike, the fluid renders the substance almost invisible, except so far as irrationality of dispersion is concerned.

As an immersion fluid Rangoon oil has some advantages; but in my hands a solution of 1 grain of chloride of gold in 1 drachm of glycerine gives fine effects.

In my first paper of 1869 I referred to the ribs of the Podura. It seems probable that the beaded structure is contained within the ribs, and serves to keep the delicate covering membrane distended.

I am of opinion, too, from recent observations with a $\frac{1}{50}$th, that the "Podura oil" circulates within this rib and amongst the beads.

For I have noticed a movement of fluid in the direction of the spurious spines, under the circumstances previously described.

In these experiments of delicate differential pressure, varying from the sixty-thousandth to the ten-thousandth of an inch, the portion first exposed to view by the recession of the oil is one of the most tangible tests of reality of structure it is possible to conceive. A rib shoots out as a rib, and a minute bead peeps forth by degrees—not *per saltum*. Bead joins bead; a short string or a small cross or chain of beads appears and disappears as the oil recedes like a wave, and again overflows the structure: exactly as the pressure is delicately increased or diminished the same particular bead or beads can be thus brought up from invisibility to a rare prominence and distinctness, adorned with colour and shadow if oblique light is used. But direct light from a plane mirror (without a condenser) reflecting the light from a white blind in sunshine answers extremely well. Otherwise a 1½-inch Ross objective forms an excellent illuminator from direct light without reflexion from a mirror.

A Tolles' $\frac{1}{8}$th with a lengthened tube and C eye-piece shows them very well, and in justice to Mr. Tolles I must say that it is a true $\frac{1}{8}$th, magnifying exactly 600 times under the same circumstances that the Powell and Lealand "eighth" magnifies 800 diameters. I am indebted to Mr. Frank Crisp (Memb. Council Roy. Mic. Soc.) for the loan of this interesting glass, marked 98° balsam angle.

I see the beads very well also with a very fine Wray $\frac{1}{4}$th.

This method of investigation, of course, as only showing the parts that are left high and dry, disposes, once for all, of the question of spurious beading, fondly ascribed to optical illusion.

It may not be uninteresting to mention that in focussing downwards with the dry Powell and Lealand $\frac{1}{8}$th. At first:—Shaded with a blue diaphragm, the beads appear dark red, some lighter red, and others approaching white. Deeper:—The shades all pass into white. Deeper still:—Dark blue appears the predominant colour of the beading. Then, lower still, these colours recur over again, apparently from the effect of a double layer lying beneath and betwixt the upper.

Careful examination shows the test-oil clings to the under surface, but gradually oozes upwards by pressure, and then by degrees, as it were, completely dissolves out the beads by obliteration. It forms small globules, giving minute diffraction rays and a brilliant focal point, according to the degree of pressure, or what is the same thing, optically, the degree of space between the upper cover and the lower slide.*

* The splendour of the definition was demonstrated by the jet blackness of cylindrical forms.

At this stage of more perfect microscopical definition it is not without piquancy

Under the continued action of the concentrating heat of the condenser the oil becomes much more fluid. The spherules look brilliant, and at one focus of a brilliant sapphire hue, at another a dull ruby red. The Tolles' glass gave an appearance of fine bead-like convexity—14-inch tube and C eye-piece.

In a paper * in the 'Monthly Microscopical Journal,' Jan. 1870, I related the phenomenon of fluid shooting between the ribs of the Podura, and almost obliterating a row of beads by the insinuation of the water through the cracked cover. I was pleased some time since to find that, subsequently, Mr. Joseph Beck exhibited at the meetings a somewhat similar appearance, caused by ingeniously breathing upon a scale; the fluid was seen to run along the grooves caused by the ribs.

I have always described the Podura as ribbed and beaded. Of course a string of minute beads approximately forms a rib. I am inclined to believe the beads or ultimate molecules fill up the corrugations really forming beaded ribs; and I deduce this fact from the black edgings frequently seen in the interrupted ribs optically forcing the so-called spines.

to refer to former beliefs. Speaking of the Podura markings, in July, 1869, a great student in Podura and other test objects wrote, p. 26:—

"Under this excessive amplitude each individual marking retains its characteristic form . . . not the most careful focussing can determine that it stands above the surface of the scale."

"Under the parabolic condenser, with a $\frac{1}{4}$th object-glass and a black field, this is a most beautiful object."

"In all other respects of interval, form, and position they are the same as under transmitted light, and we are equally unable to prove that they exist in the form of projections."

"By transferring the investigation to a scale that has been ripped open or torn across, nothing can be learnt. The tear continues without interruption clean through the markings, one half of which will be left on one piece and one half on the detached one, and no snags or projections can be seen on the clear edges of the suture."

Comparing this with recent statements, there is reason to believe that definition is now greatly improving.

* Page 13. . . . "the beads look just like rows of peas in a pod. But the water is now insinuating itself, and bright spots four times the size of the beads arise like blebs of a bright green colour. One rouleau vanished instantly." "I had the satisfaction of exhibiting several times to Mr. Reade the upper and lower beads of the test-scales he brought with him." (Rev. J. B. Reade, President.)

## III.—*Note on the President's Remarks on The Searcher for Aplanatic Images, as to the Principles upon which it acts.*

By Dr. Royston-Pigott, F.R.S.

(*Read before the* Royal Microscopical Society, *March* 4, 1874.)

I regret that the paper in the 'Philosophical Transactions' on this subject has not been printed in the Journal, as also the two quarto plates. (I have placed fifty copies at the disposal of the Society.)

The searcher gives a new means of balancing spherical and chromatic aberrations.

It is on a large scale precisely what the adjusting screw-collar of an objective is on a minute scale. The collar separates the front lens by thousandths of an inch. The searcher traverses inches.

Compensating lenses may be applied at many different places. Thus, Mr. Wenham's improved object-glass depends principally upon the compensation of the back lens or posterior glass of the combination for balancing the aberration introduced by the front sets. If he were to make this back lens traversing on the principle of the searcher, he would have a wider choice of balancings.

The thick front introduces very considerable aberration; this is corrected more or less perfectly by the posterior lens.

Now the aberration of a lens varies rapidly with the diameter of the aperture. If the searcher be placed in a more distant position, a smaller pencil engages its surface, and its aberration is diminished.

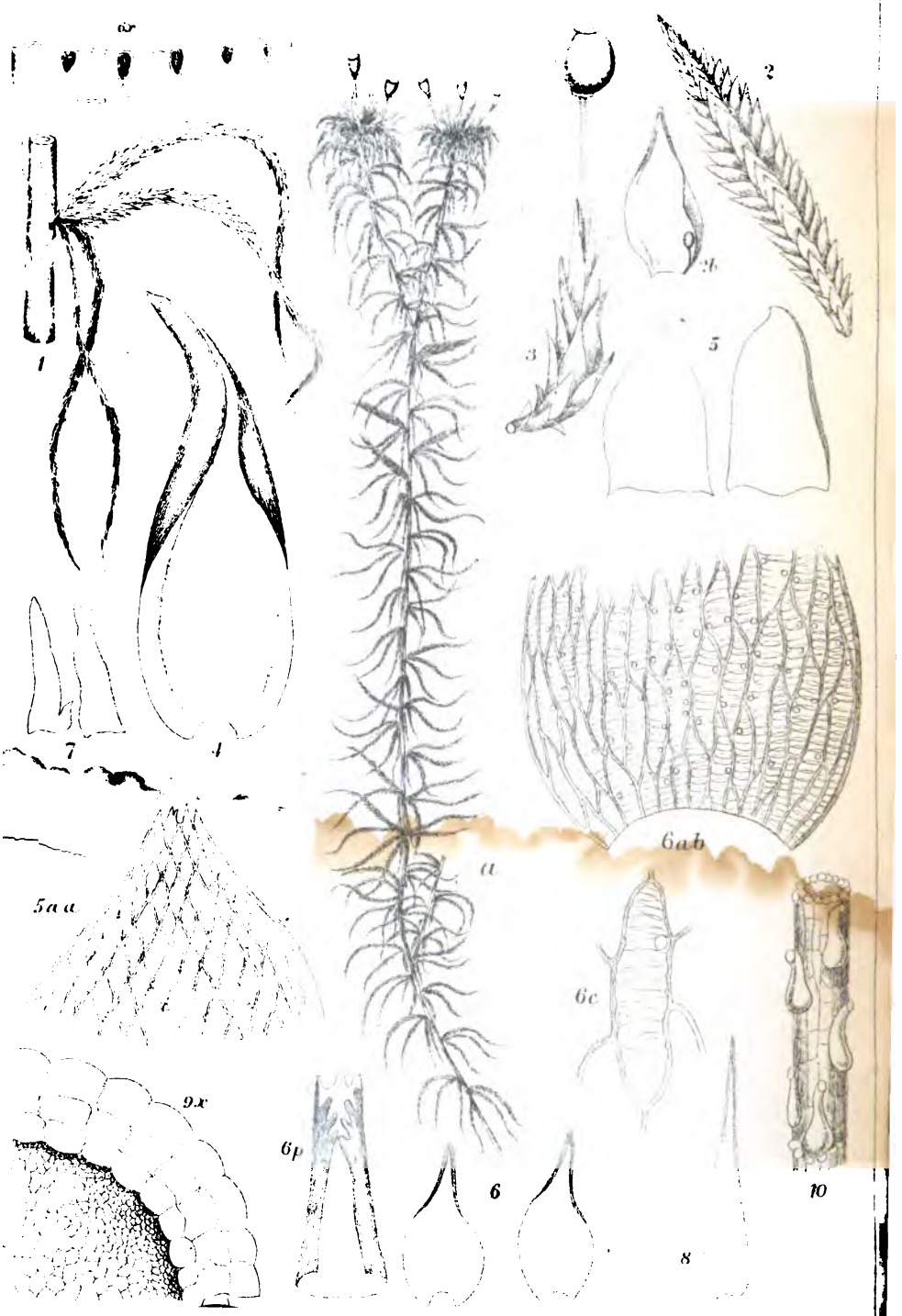
If a short tube be used—say 5 inches instead of 10—the object-glass becomes violently under-corrected. An over-corrected lens, properly chosen, may be found to correct this. But as it is impossible practically to apply an infinite number of differently corrected lenses, the more simple expedient of placing a given over-corrected lens nearer or farther from the objective answers precisely the same purpose.

By patiently moving the searcher into different positions, and also altering the screw-collar, some particular position is realized which just balances the objective residuary error in the most satisfactory manner of which the workmanship of the objective is capable.

*In doing this, miniatures of known objects formed by a first-class objective are the most satisfactory* as a thermometer bulb or watch-dial, or mercury globules of the size of large shot illuminated by the sun and placed at 10 inches below the stage,* form magnificent tests, and obviate the difficulties attending side illumination on the stage of globules of the smallest kind; a good objective

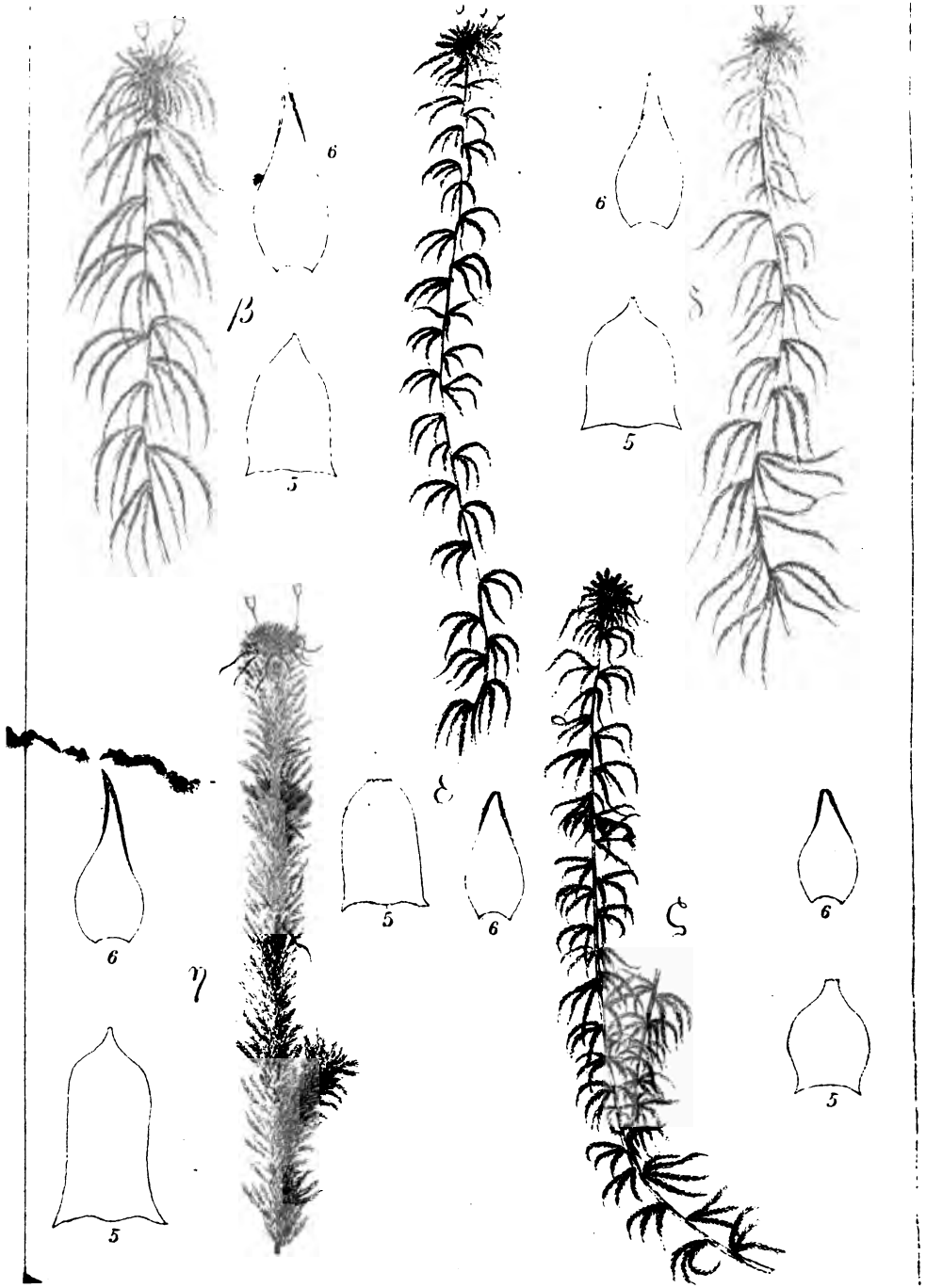
* See 'Phil. Trans.'

used as a condenser being capable of forming a test-image, which at once declares, like a telescopic test, its conditions beyond any possible doubt: because its appearance when magnified up can at once be compared with the tangible reality as it appears to the eye of the observer. In each case it is advisable to magnify the miniature so as to appear as large as the real object. This is a totally different thing from a disputed scale of an insect which we cannot see with the naked eye.




Braithwaite del. et lith.

Erh. serrifolium.




Swaite del ad nat. West & Co. lith.

Sph. acutifolium Var[s]

## IV.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Group D. *Cuspidata.*—Plants rather rigid, loosely matted. Branch leaves erecto-patent, narrowly lanceolate, acute or much acuminate, truncate and toothed at apex, margin more or less bordered, involute at point.

### 11. Sphagnum acutifolium Ehrhart.

Plant, Crypt. exsic. No. 72 (1786).

Plates LVII. and LVIII.

Syn.—*Sphagnum palustre, molle deflexum, squanis capillaceis.* Dillen. Hist. Musc. p. 243, tab. XXXII. fig. 2A. (1741).—*Sph. palustre* β. L. Sp. Plant. p. 1569 (1763).—Hedw. Fund. Musc. I. T. III. fig. 13–15 (1781).—*Sph. acutifolium* Ehrh. l. c.—Schrader Spicil. Fl. Germ. p. 58 (1794).—Schultz Fl. Stargard. p. 275 (1806).—Web. & Mohr, Bot. Tasch. p. 73 (1807).—Schkuhr, Deutsch. Moose p. 15, t. 6 (1810).—Voit, Musc. Herbip. p. 12 (1812).—Schwägr, Supp. I. P. I. p. 15, t. 5 (1811).—Röhling, Deutsch. Fl. III. p. 36 (1813).—Hook. & Tayl. Musc. Brit. p. 4, t. IV (1818).—Funck, Taschenh. p. 5, t. 3 (1821).—Zenk. & Dietr. Musc. Thuring, F. 1, No. 20 (1821).—Nees Hsch. & St. Bry. Germ. I. p. 19, t. III. fig. 8 (1823).—Hüben, Musc. Germ. p. 28 (1833).—De Notaris, Syll. Musc. Ital. p. 297 (1838).—Dozy & Molkenb. Prodr. Fl. Batav. p. 78 (1848).—C. Müll. Synop. I. p. 96 (1849).—Wils. Bry. Brit. p. 20, T. IV (1855).—Hartm. Skand. Fl.—Schimp. Torfm. p. 56, Tab. XIII. (1858).—Synop. p. 672 (1860).—Lindb. Torfm. No. 5 (1862).—Berkel. Handb. Br. Mosses, p. 307, Pl. 2, fig. 4 (1863). Russow, Torfm. p. 37 (1865). Milde, Bry. Siles. p. 381 (1869). *Sph. capillifolium* Swartz, Act. Holm. 1795, p. 281.—Bridel, Musc. Rec. II. pt. 1, p. 24 (1798). Sp. Musc. I. p. 16 (1806). Mantissa, p. 1 (1819). Bry. Univ. I. p. 11 (1826).—Hedw. Sp. Musc. p. 28 (1801).—Smith, Fl. Brit. p. 1146 (1804). Eng. Bot. t. 1406 (1805).—Turner, Musc. Hibern. p. 6 (1804).—P. Beauv. Prodr. p. 87 (1805). *Sph. capillaceum* Swartz, Musc. Suec. p. 18 (1798).—Wahlenb. Fl. Lapp. p. 302 (1812). Fl. Carpat. p. 333 (1814). *S. capillifolioides* Breutel, Bot. Zeit. 1824, p. 438.—Brid. Bry. Un. I. p. 751. *Sph. Ascherbachianum* Breut. l. c. p. 439.

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### EXPLANATION OF PLATES.

#### Plate LVII.

Sphagnum acutifolium.

*a.*—Plant of the typical form.
1.—Part of stem with a branch fascicle.
2.—Catkin of male inflorescence. 2 *b.*—Bract from same with an antheridium.
3.—Fruit and perichætium. 4.—Bract from same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Leaves from middle of a divergent branch.
6 *a b.*—Areolation of base. 6 *x.*—Transverse section.
6 *p.*—Point of same. 6 *c.*—Cell from middle × 200. 7.—Basal intermediate leaves. 8.—Leaf from a pendent branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

#### Plate LVIII.

Sphagnum acutifolium.

β.—Var. deflexum.
ε.— „ tenellum.
δ.— „ purpureum.
ζ.— „ fuscum.
η.— „ luridum.
5.—Stem leaves. 6.—Branch leaves.

*Monoicous, in soft tufts, pale green, more or less tinged with purple.* Stems slender, dichotomous, pale externally, *cortical cells generally without pores, in* 3–4 *strata,* the woody zone purple. *Fascicles of* 3–5 *branches, of which* 2–3 *are divergent,* 1–2 *pendent, all more or less attenuated at points;* their retort-cells flask-shaped, sub-cylindrical, with the apex perforated and slightly recurved. *Cauline leaves erect, ovate acuminate, concave with the margin incurved, minutely auricled at base,* 5-*toothed at apex;* hyaline cells of the middle base hexagono-rhomboid, divided by one or two oblique partitions, without fibres or pores, the upper also divided, and often slightly fibrillose, lateral very narrow and forming a broad border, gradually decreasing in width toward apex. *Basal ramuline leaves minute, ovate, median ovato-lanceolate, erecto-patent deeply concave, uppermost narrowly lanceolate, all toothed at the slightly truncate apex, and with the margin involute in the upper third; hyaline cells confluent at back,* with a few large pores and annular fibres; border extremely narrow of two rows of long thin cells; *chlorophyll cells obtusely trigonous, interposed between the hyaline on the concave surface of the leaf.*

Male amentula usually purple, clavate acute, the bracts in five rows, ovate, acute. Capsules numerous, usually clustered in the capitulum; peduncle moderately elongated, the bracts numerous, straw-coloured or reddish, lowest broadly ovate, acuminate, concave, median oblong, narrowed at apex, uppermost elongated, convolute; the hyaline cells narrower and more solid than in the cauline leaves, with 2–3 partitions but no fibres or pores. Spores ferruginous.

* Varieties more or less tinged with purple.

*β. deflexum.* Schpr.

Plants shorter, densely matted, with close set fascicles. Branches flagelliform, all decurved, pink and green, the points whitish. Branch leaves closely imbricated, longer and narrower.

*γ. lilacinum.* Spruce ms. (*roseum* Limpricht).

Plants robust, loosely tufted, fine rosy red, suffused with violet. Branches erect and divergent, loose. Stem leaves rounded at apex; branch leaves strongly involute, 4–5 toothed.

*δ. purpureum.* Schpr.

In dense cushioned tufts, usually entirely purple; plants short slender, with a dense capitulum, closely ramulose. Branches short curved, with obtuse leaves. Perichætial and stem leaves sometimes fibrillose.

*ε. tenellum.* Schpr.

Laxly tufted, slender and elongated with distant fascicles, pale green and red. Divergent branches arched, with short obtuse leaves. Stem leaves lingulate, broadly bordered, fringed at the rounded apex.

** Varieties of a fuscous or pale colour.

ζ. *fuscum.* Schpr.

In matted tufts of an ochraceous brown colour; stems slender dark brown, densely and uniformly ramulose. Divergent branches short, incurved, their leaves short and concave with a rounded, toothed point. Stem leaves small, with a rounded, toothed apex.

η. *luridum.* Hübener.

In dense tufts of a dirty green colour above, fuscous below. Branches very densely crowded, all erecto-patent and of equal length; leaves closely imbricated, acuminate strongly involute at points. Stem leaves large, elongated linear, acutely pointed.

ϑ. *patulum.* Schimper.

Plants robust, loosely tufted, entirely of a pale green colour. Stem leaves elongated acute, with the cells in the upper third fibrillose. Branch leaves elongated, patent, laxly incumbent when dry.

ι. *arctum.* Braithw.

Stems fragile 1–2 in. high, in very dense yellowish-green cushions. Stem leaves narrowly bordered, with the cells in the upper two-thirds fibrillose; branches densely placed, short, ascending, with laxly areolate obtuse leaves.

κ. *alpinum.* Milde.

In dense snow-white tufts; all the branches erecto-patent, longish. Stem leaves long, fibrillose, with a faint rose tint.

λ. *plumosum.* Milde.

Tufts very lax, long and floating, reddish brown, with long lax-leaved branches. Stem leaves very long, toothed; branch leaves elongated, involute, with a broad 7–8 toothed point.

Hab.—Heaths and bogs, common. Fr. July. β, in peat bogs and pine woods. γ, Terrington Carr. (Mr. Spruce); Fowlsham Moor, Westmoreland (Mr. Stabler). δ, ε, ζ, Alpine bogs. η, Ben Lawers (R. B.). ϑ, grassy shady places in moorlands. Darnholme near Whitby. ι, Brandon mountain, Kerry (Dr. Moore). κ, Alps (Milde). λ, Remschied near Düsseldorf (Döring).

This most variable species is generally distributed, and found on heaths and in woods, in the plains as well as on the mountains. The stem leaves vary in the amount of threads in the cells, and also in the width of the border, and size of teeth at the apex, (these sometimes becoming broken up into a slight fringe), but always differ from those of *S. strictum* in being narrowed to a point.

The leaves on the lower half of the divergent branches, present generally a marked difference in their cells, these in the lower two-thirds of the leaf are large, and have a few large pores, but in the upper third the cells become very small, and have several small pores; towards the point of these branches, and on all the pendent branches, the leaves become narrowly lanceolate, and the cells are uniformly lax and with equal large pores throughout the extent of the leaf.

I have assumed as the type of the species the form called *robustum* by Blandow, and although so many named varieties are brought forward, several other peculiar forms appear equally entitled to the rank. Occasionally the majority of the divergent branches are converted into male amentula which stud the stem throughout its whole length, and render the plants conspicuous by their rich purple colour. That the varieties do not depend on conditions of soil is proved by the fact, that they often grow close together, or even in the same tuft, but each maintaining its peculiar characters.

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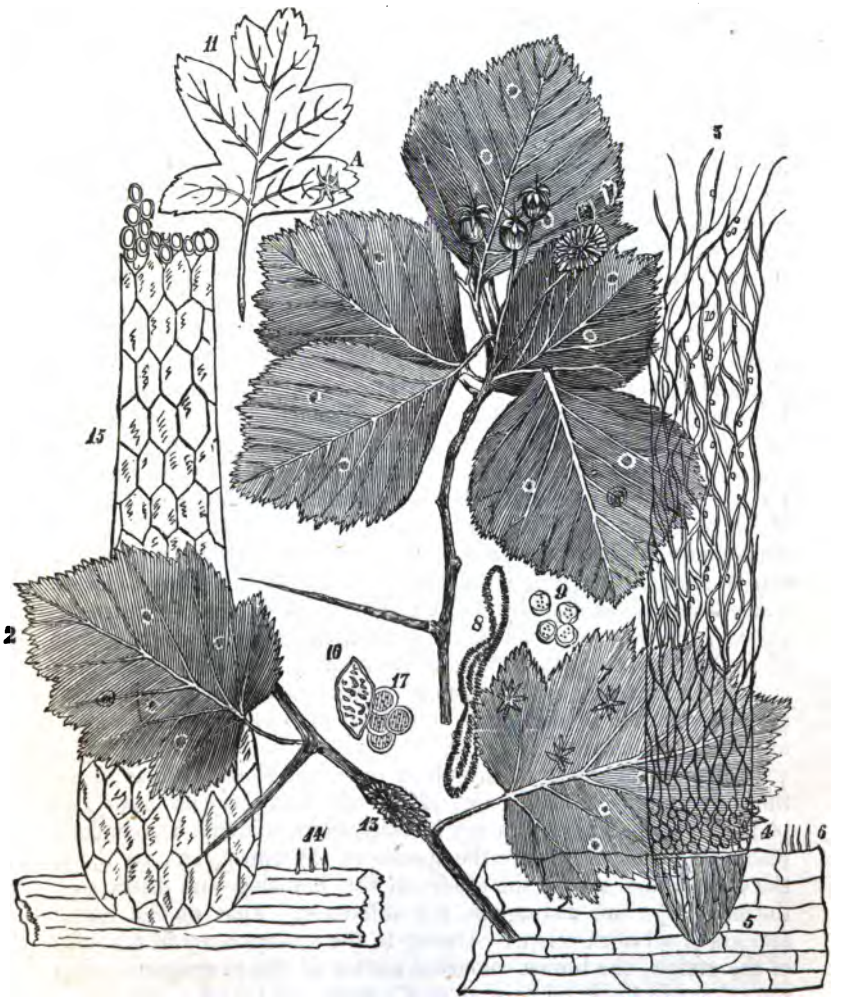
V.—*The Fungus of the Hawthorn — Rœstelia lacerata, Tulasne; Æcidium laceratum, Grev.* By Thomas Taylor, Superintendent of the Microscopical Department of the Commission of Agriculture, U.S.A.

Dr. Greville, in his 'Scottish Flora,' p. 209, vol. iv., describes this fungus as it was known in 1826. He says that it is found on the nerves and petioles of the leaves, on the fruit, and even on the young branches of the hawthorn (*Cratægus oxyacantha*) in summer and autumn everywhere; and M. A. Cooke observes that it is found on the under surface of the leaves and on the petioles and fruit of the hawthorn, and is common from May to June in the United States.

My attention was called last year to the prevalence of this fungus on the hawthorn plants on the grounds of the Department during the months of July and August. This year it has also appeared. I first observed its presence in the month of July, although it may have appeared in June preceding. At this time, September 20, the fungoid forms are decaying. Nearly every variety of the hawthorn is affected, especially *C. punctata* and *C. tomentosa*.

The Washington evergreen hawthorn plant *C. pyracantha*, Pers., seems not to be attacked by any species of fungus of the order *Æcidiacei*. Judging from my observation I deem it an error to suppose that *Rœstelia lacerata* attacks either the branches or fruit of any variety of the hawthorn. We have many varieties of the hawthorn growing on the grounds of the Department, but in no case have I found *Rœstelia lacerata* on their fruit or branches. This species is confined to the leaves in every instance, and the petioles thus far are exempt from its attacks. On making my first observations and dissections of the orange-coloured fungus, seen so frequently on the branches and fruit of hawthorn bushes, I was much disappointed on finding that the colour, structure, &c., of the peridium and spores did not agree with that given by mycologists;

but on making search to ascertain if any other similar genus or species of the order *Æcidiacei* existed on the hawthorn *C. oxyacantha*, or on any of the numerous varieties of this hedge plant, with the view of accounting for the discrepancy, I found, on turning up the leaves having orange spots on their upper surface, true peridia (sacks) of




*Rœstelia lacerata*, Tul.; *Æcidium oxyacantha*, Pers. Under a power of about 90 diameters the general character of the peridia is seen. They are densely aggregated, elongated, submersed, pale brown, irregularly torn. The sporidia are copious; thus far

agreeing with Greville's description. The circular spots on the leaves, Nos. 1 and 2, indicate the general points of growth of this fungus. I find it frequently on the leaf-ribs and terminal points of the leaves, and very often dispersed over the smooth parts of the leaf; sometimes, although rarely, the peridia are on the upper surface of the leaves. 3 represents the peculiar formation of their structure, which resembles network. At the juncture of the leaf (see 4) the cells of the peridia are nearly round; at 5 oblong. From 3 to 4 the cellular structure is of a light vandyke brown; at 5 a pale yellow. I am aware that the structure of the peridia, as described by me, varies in some respects from that by Greville and others, which shows the importance of photographing so minute objects. I have presented sections of these for future use. 6 represents the appearance of the peridia as seen by the naked eye; 7 their general arrangement and their groupings on the leaves; 8 three cells, showing the parts of which the peridia are composed when magnified about 125 diameters; 9 the spores contained in the bottom of the peridia magnified 125 diameters; 10 represents the openings or meshes; 11 a leaf of a variety of *C. oxyacantha.* On one of its lobes, at A, is represented a cluster of peridia of *Rœstelia lacerata.* 14 represents the peridia of a species of *Æcidium* as they appear to the naked eye, heretofore undescribed as a parasite on the hawthorn, and may have been confounded with that on the leaves; 15 a very highly magnified view of one of them, the cells of which are magnified 125 diameters; 16 one of the cells somewhat more highly magnified. They are not always regular in construction, although generally of the form given. They separate easily from one another by slight friction. 17 represents spores of an orange colour, with which the peridia abound, and which consist of at least three parts: first, a transparent outward cell, which contains an orange colouring-matter, within which may be seen germinal matter in the form of dark spots. The spores are magnified 125 diameters.

All standard works on mycology represent *Rœstelia lacerata* as the only fungus of the order *Æcidiacei* that attacks hawthorn plants; but judging from my investigations, it holds a secondary place. So conspicuous are the species of the two genera on them, the one on the leaves, the other on the branches and fruit, that the naked eye can distinguish the difference. That on the leaves appears of a brown colour. Owing to the transparency of the cells of the peridia, the brown colouring-matter of the protospores being seen through them, while that on the fruit and branches appears of a rich orange, owing to the colour of their protospores. Although of the same order they differ in genus and species.

It is of much importance to ascertain as far as possible the conditions of growth favourable or unfavourable to this order. Its

presence on plants is highly destructive to them, and has proved one of the most serious obstacles to the cultivation of the hawthorn as a hedge plant in the United States. Forty-seven species of *Æcidium* and three of *Rœstelia* are reported by M. C. Cooke. In relation to the ravages of this order of fungi P. H. Foster, proprietor of Babylon Nurseries, Babylon, Long Island, writes to the Commissioner of Agriculture, on the 1st of August last, as follows:—

"I send you a specimen of a disease which occurs on some American white-ash trees, which I imported from Flushing, New York. I have noticed the disease on them during the last two seasons. It first makes its appearance early in the season on the leaves, and finally attacks the young wood, as may be seen on the specimens enclosed. It is evidently of fungoid origin. I have many thousands of plants of American white-ash, from two to three years old, planted in my nurseries, none of which are affected with this disease. I have also some European ash, which appear to be very susceptible to it. I wish to obtain a remedy. The loss of so valuable a timber-tree would be too great for our country to bear."

The Department will at an early day commence a series of experiments, having relation to the best mode of treatment of plants affected with this fungoid form of disease. The results of the experiments will be published in the monthly reports.

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## VI.—*Points in the Histology of the Human Kidney.*

By R. Branwell, Brighton.

The great and varied importance of a correct histology of the kidney is fully recognized on all sides; in the interest of one of its chief bearings, *viz.* the pathology of the organ, I beg space in the Journal for some observations referring to the structure and function of the malpighian body, and the convoluted urinary tubule.

The well-known doctrines of Bowman's school in relation to these structures are still held valid by renal physiologists; and more than that, they are practically treated as indisputable.

And yet every independent inquirer must have had difficulty in realizing the truth of some of Bowman's descriptions; so much so indeed, that assent to them is probably often compelled, by the weight alone of that great teacher's deserved authority.

Thus the malpighian body is described as a flask-like expansion of the upper end or commencement of the urinary tubule (Bowman's capsule), which receives within it the afferent artery; this artery as then breaking up into branches, which after running a short course as arterioles or capillaries, return and again reunite to form the efferent vein; the connection between the artery and vein con-

sisting in this way of a number of small looped vessels called the malpighian tuft.

According to this description the malpighian body consists of two structures, and two only, *viz.* Bowman's capsule, and the free tuft of vessels it encloses. But on section of the fresh and unmanipulated kidney, we have in fact in addition to these a third, forming too the largest, most prominent, and most obvious feature of the whole. This additional structure is composed of cells, and in appearance is hardly if at all distinguishable from the glandular epithelium lining the convoluted tubes. Our section of the malpighian glomerulus in short presents a picture, not simply of a capsule containing vessels, but one composed mainly of a spherical-shaped cellular body; it is within the tissues of this body that the vessels ramify and form their loops as described, whilst its surface is closely invested by the so-called capsular membrane in question.

The function of the malpighian body has been deduced from its supposed simple structure; and nothing could be more natural than the idea that the tuft served, by a process of exosmosis, to pour off the watery portion of the urine, whilst the capsule collected and transmitted the same to the tubule in continuation with it. But in presence of the above-described additional cellular body, it is clear we have to seek anew the duty which must be assigned to the organ; the functional physiology, that is to say, of this portion of the tissues of the kidney, together with its pathological significance, have in fact yet to be discovered.

The current descriptions of the convoluted tubuli uriniferi like those of the malpighian body, would seem to require an almost thorough revision. The accepted view is, that the tube of basement membrane is lined to not more than a third of its depth, by glandular or secreting epithelium in the form of distinct cells; leaving thus a central open canal for the passage of the secretion.

It will be remarked that this central canal fits in well with Bowman's idea of the function of the malpighian body, and perhaps is essential to it. But upon any other ground than this, it would be difficult to imagine how the conception originated; for there is nothing more certain than that no such canal can be seen; on the contrary, in the words of Ludwig, the "tube is filled by a pulpy granular mass." Nor is it the fact that this mass has a defined character of distinct cells; it is emphatically an undifferentiated though nucleated mass.

It is true that Ludwig adds "this pulpy mass presents numerous fissures, which however lie at very irregular distances"; and he gives a figure exemplifying that view. These fissures will probably be claimed as representing the central canal.

I have not been able to verify the presence of these fissures; and after much patient examination have come to the conclusion

that they have no existence, and that no trace of them or any other kind of opening whatever is to be found. The tube is therefore actually completely filled by the pulpy granular mass in question, so that no canal, fissure, or space of any kind exists for the passage of the secretion.

Doubts may occur to some in the absence of the central canal or other space, as to the mode in which the secreted urine passes along the tubuli uriniferi: but there is a simple explanation at hand which not only clears up the difficulty, but at the same time indicates more satisfactorily than hitherto the *modus operandi* to some extent, of another important function of these organs.

Let us suppose that the water finds its way from particle to particle along the tube by capillary attraction, or some analogous force, and we shall have at once an obvious and sufficient method of transit. And assuming that the pulpy mass filling the tube is permeated with water in this way, it is probable the washing away of the saline matters it is its function to separate, would be more easily and thoroughly accomplished than would be the case if the water merely ran over its surface, as in the presence of any form of canal.

Observation and theory therefore are so far at one in ignoring the orthodox teaching upon these points in renal histology.

But fortunately observation only is required to settle these questions, and everyone may submit them to the test of the microscope for himself. The only conditions stipulated for are, that the section of kidney should be fresh, unmanipulated, and examined in glycerine.

I think it essential to insist upon this simple method of examination, because, while it is sufficient, it obviates all doubt and silences all question as to the tissues being seen as nearly as possible in their natural condition.

This point of departure in every histological inquiry is a *sine quâ non;* and it is not until we have placed ourselves in full possession of the facts relating to general characteristics as they may be learnt from such modes of procedure, that we may venture gradually and by successive steps to the more elaborate manipulation that may be required for the display of any new tissue or organ in its minuter detail.

It was probably due to such want of precaution as this that the malpighian body was, and still is, regarded by Bowman and his school as composed, except as to the capsule, entirely of a free tuft of capillary blood-vessels. The vessels of the kidney, apparently without preliminary examination of the organ, were at once injected, and with such violence was the process conducted, that in some instances the injecting fluid was actually forced out of the capillaries into and along the entire length of the tubuli uriniferi!

The necessary result of such work as this was in the first place greatly to distend and enlarge the vessels, and in the next, to encroach on all sides upon and to compress the cell tissue in which they lie imbedded. The double effect upon the examiner of objects so prepared would naturally be on the one hand to magnify the importance of the vessels, whilst on the other that of the cell tissue would be diminished and even hidden altogether.

Again, if we consider that the injecting fluid by which this state of things was brought about was opaque, we shall get still further insight into the process by which the tissue, now recognized as composed of cells, originally came to escape observation; for in addition to the circumstance of their great enlargement the vessels in such a state of things could only be examined by reflected light—a mode unfitted from its very nature to deal with the cell tissue, as also because comparatively low powers only could be used for the examination of specimens so prepared.

It might *a priori* reasonably be thought that the introduction and employment of transparent injections would effectually correct most of the faults in this category; but unfortunately perfect injections of the capillaries of this kind, through the tendency to transfusion of the fluid used for the purpose, are very difficult to make; and most of those seen display a blotted and confused outline, that I cannot doubt has served to confirm and perpetuate, instead of discovering the faults in question.

There are still many other sources of error in prosecuting these inquiries, and not the least among them lies in the practice of substituting the organs of the lower animals for those of the higher. I wish carefully to avoid being understood as depreciating in the least degree the value of comparative histology; it is the tendency only that I point to, existing in instances of difficult research where it may be necessary to appeal to lower and simpler organizations, to regard these latter not only as representing in a general way more developed and complicated structures, but actually to look upon them as if they were almost, even if not quite, identical with them.

It is not necessary to extend this enumeration of possible and probable sources of misinterpretation, further than to notice the effect of hardening processes upon the tissues of the kidney.

To procure sections sufficiently thin for microscopic examination, it is a very general practice to subject the organ to the action of alcohol. It is certain, however, that after such treatment the kidney in many important respects no longer presents its natural appearance, and notably so with regard to the tubuli uriniferi; for whereas, as already stated, when examined in simple and indifferent media they present no central canal, nor any the least trace whatever of it, by exposure to the action of spirit they may often be made to

present such an appearance, more or less exactly as it is given in the pretty and complete pictures which pass for illustrations of renal tissue in the books on the subject.

It might be asked, Why should the hardening and contracting effect of alcohol on the epithelium of the tubule have the appearance of a central canal as a consequence, and not rather a breaking up into irregular forms? The best answer to such a query would be to point to the facts as already indicated. But if a reason must needs be furnished, it would be easy to find one in the circumstance that the epithelial tissue next the basement membrane is younger, nearer the sources of nourishment, more vigorous therefore and able to maintain its cohesion; whilst to that in the centre the contrary conditions apply, and hence in contracting it is this portion which must yield. It follows also as a consequence of the circular character of the tubule, that its contents will contract equally towards the circumference, leaving an empty ring in the centre.

It is clear therefore that the kidney has hitherto been made the victim, so to say, of the very means employed for its elucidation; for while on the one hand the current descriptions abound with new and manufactured appearances, on the other it is unusually liable to the errors which, as above pointed out, but too frequently ensue from the substitution of the organs of the lower animals.

To return to the actual description before us, it will be remembered that the more recent investigators are agreed that there is at least a cellular element in the composition of the malpighian body. Thus some consider the capsule to be lined with cells; and others state in addition that there is a layer of cells covering the vessels; the main feature, however, of Bowman's idea, that the organ is made up of a tuft of vessels enclosed in a capsule, being still held good by every writer.

In opposition to the views of Bowman, as also to those of more recent date, it has been already seen, that I venture to assume and maintain the malpighian body to be primarily and essentially cellular, and that its vessels are secondary and subordinate; that the cell structure is not limited to the lining of the capsule and the covering of the tuft of vessels, but that it composes the bulk and corpus of the organ, and that the vessels instead of being free and independent, ramify in the mass of cells and contribute to their function; much in the same way in fact as the main principle of the liver lobule is cellular, while an especial system of vessels is necessary to its office.

The very simple proof of this proposition is, that at whatever angle the glomerulus is cut through, or whatever segment of it may be removed, the portion remaining invariably shows the same cell structure. In a thin section of the uninjected and otherwise unmanipulated and uninjured kidney these bodies are necessarily

presented to the observer in every possible variety of segment, and yet there is absolutely no difference whatever in their appearance.

This circumstance is obviously inconsistent with the view that the cells form either one or two thin laminæ, one of which is said to line the capsule, and the other to envelop the vessels; the former would be removed from the surface by the act of section, and therefore no longer seen; the latter would show the outline of a filmy crust of cells surrounding the enclosed vessels — an appearance opposed to the actual facts and conditions of the parts.

The malpighian body and the urinary tubule constituting as they do together the proper structure of the kidney, errors in their description like those under consideration, involve nothing less than a corresponding misconception of the physiology of the organ, and as a further consequence of that of its pathology; this brings me back to the point from which this communication started.

It is not necessary here, nor is this the proper place to examine the history of disease of the kidney; but it may with propriety be stated that a very important part of it deals with the function of the malpighian body as hitherto conceived; and that the presence of casts of the tubuli uriniferi in the urine is taught as indicating a variety of degrees of departure from the healthy condition of these tubes. Though this latter doctrine is indisputable in a general sense, it is clear it must be modified in so far as certain of the imports of these casts are based upon the existence of the central epithelial canal; since they must be fallacious if the legend of that canal is itself the fable I have shown it to be.

Questions of more than ordinary interest, both to histology and to the theory and practice of medicine, are involved in these issues; and such being my justification in bringing the subject forward, I trust that these strictures may be regarded as a challenge to those most capable, to further investigation of the organs.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Animal Distribution of Hæmoglobin.* — This is a subject of some importance, especially when viewed from Mr. Sorby's aspect. However, M. Quinquand, who has investigated its distribution through the animal kingdom, has not, we believe, made any inquiries in this direction, though he has published tables of its distribution in animals. His conclusions are thus formulated:—1. The progressive diminution of the quantity of hæmoglobin contained in the same volume of blood follows, in general, the degrees of the animal scale; but the blood of Primates does not contain most. 2. The blood of young animals contains less hæmoglobin than that of adults. In many species the placental blood contains as much as that in the general circulation. In old age the quantity diminishes. The curve of variation presents a slight fall at first, corresponding to the first days of extra-uterine life; then it rises gradually (in the case of man) to twenty-five years, and continues horizontal till fifty; after which it slowly falls again. 3. The proportion of hæmoglobin in birds is much smaller than in mammalia. The weight of the corpuscles is slightly greater in birds, but the corpuscles of mammalia contain three times less albuminous matter. 4. The influence of sex is observable. Females have generally less hæmoglobin than males. 5. The lymph of crustacea contains four to five cubic centimètres per cent., while ordinary water contains, at its maximum of saturation in winter, one cubic centimètre per cent., and in summer only six-tenths of a cubic centimètre.

*The Embryology of Limulus* was detailed by Dr. L. S. Packard, at the meeting of the American Association. He said that in a recent paper on the Embryology of Limulus, published in the Memoirs of the Boston Society of Natural History, he stated that the blastodermic skin just before being moulted consisted of nucleated cells; and also traced its homology into the so-called amnion of insects. This summer he has, by making transverse sections of the egg, been able to observe in a still more satisfactory manner these blastodermic cells and observe their nuclei before they become effaced during or after the blastodermic moult. On June 17 (the eggs having been laid May 27) the peripheral blastodermic cells began to harden, and the outer layer—that destined to form the amnion—to peel off from the primitive band beneath. The moult is accomplished by the flattened cells of the blastodermic skin hardening and peeling off from those beneath; during this process the cells in this outer layer losing their nuclei, and, as it were, drying up, contracting and hardening during the process. This blastodermic moult is comparable with that of Apus, as he has already observed, the cells of the blastodermic skin in that animal being nucleated. The paper set forth that while the process above described resembled features in the development of the scorpion, and thus strengthened the supposition of Burmeister, that the Limulus is related to the spiders, nevertheless other features which Prof.

Packard pointed out led him to believe that the Limulus is related to the lower crustaceans, but is, like all the earlier or palæozoic types, comprehensive or synthetic, comprising certain features belonging to higher forms, while yet holding its proper affinities with the lower ones. He also confirmed the brilliant researches of A. Milne Edwards upon this representative of an ancient type.

*The Lymphatics of the Skin in Man and Mammalia* have been splendidly explored by Herr Dr. Neumann, who has recently published a most valuable work with eight chromo-lithographs on this subject. His book is reviewed at length by Dr. L. A. Duhring, in the 'Philadelphia Medical Times,' from which the following account is taken:—

"The plan adopted by Neumann for injecting the skin was a modification of the methods of Hyrtl and Teichmann. The epidermis and the corium having been well macerated in a mixture of alcohol, acetic acid, and water, a fine-pointed needle was thrust into the skin to the depth of a line or less. Into this little hole a delicate tube was inserted, and the injection then made by means of a small brass syringe. Two mixtures were used for the injection; one being composed of a carmine solution with glycerine, and the other carbonate of lead rubbed up with glycerine." After going at length into the subject, the reviewer concludes by summing up the results as follows:—

"1. The lymphatics of the skin present an enclosed tubular system, with independent walls, whose interior is lined with flat epithelium. These walls are nowhere interrupted by openings. There exists therefore no communication with the so-called juice-canals, or with other interspaces of the skin. Neither can spaces anywhere between the epithelium be noticed, not even in examples of disease where there exists an enlargement of these vessels.

"2. The relation of the blood and lymph vessels is only constant to the extent that the former are always found much nearer the surface than the latter. The branches of the lymphatics, together with their meshes, are found spreading themselves in the deeper tissue in all directions. Nowhere, however, within a lymph-tubule could a second vessel be detected; so that there can be no ground for considering the question of invagination.

"3. The lymphatics form two close and separate networks in the corium, the deeper being the more extensive of the two. Their walls are markedly capable of extension. The more superficial vessels are in general thinner; the deeper ones are thicker, and, like the first, are to all appearances without valves. Only among the subcutaneous vessels is it possible to demonstrate the valves plainly.

"The larger lymphatics possess a number of branches with blind endings, which are of variable calibre. The lymph-vessels make their way into the papillæ of the skin, partly in the shape of single tubules, and also in the form of loops.

"4. The appendages of the skin, as the hairs, hair-follicles, and sweat-glands, possess their own lymphatic capillaries situated about their periphery, but they do not enter into the follicles."

*A Medal from the Royal Society to Professor Allman, F.R.S.*—The President of the Royal Society in conferring the medal said that it

was awarded to Professor Allman for his numerous zoological investigations, and more especially for his work upon the Tubularian Hydroids. The subject of these labours is one upon which few persons are qualified to enter; and the Council are impressed with the delicacy of the work and the value of the scientific results.

*Trichormus Thompsoni* (?) *in America.*—At a recent meeting of the Microscopical Section of the Academy of Natural Science of Philadelphia, Dr. J. Gibbons Hunt exhibited specimens of, and made an important verbal communication respecting, the curious alga which polluted the reservoir of the Camden Waterworks last summer. During the course of his remarks he observed:—"In July last the water in the basin at Camden, New Jersey, was found to be unfit for use. When drawn from the hydrants it was offensive to both taste and smell. On examining this water with the microscope, I found in it a plant, belonging to the Nostochaceæ, diffused in great abundance through the fluid in gelatinous masses of an opalescent or faint olive-green colour. These jelly-like masses were much broken up, indeterminate in form, and enveloped innumerable spiral and brittle filaments, each having from three to fifteen turns. Cells of two kinds make up the filaments of this plant. Several subquadrate cells, about $\frac{1}{2000}$th of an inch in diameter, are arranged in linear series; then, at nearly regular intervals, globular cells—perhaps heterocysts—of equal size, and about the same diameter as the other cells, are interposed. Both kinds of cells are filled with granular contents. Owing to the extremely brittle character of the filaments, it was impossible to tell how many spirals completed an adult plant. If placed in pure water all the cells became quickly separated, and the ripest exploded like miniature bombs, scattering their granules all round. This made it very difficult to preserve a specimen. By using a medium of the same density as the gelatinous water, I have succeeded in preserving a slide of this interesting plant, which I exhibit to the Section, quite unaltered in appearance. It is possible this plant is the same that Mr. Thompson found in Lake Ballydrain, near Belfast, Ireland, and described as *Trichormus Thompsoni*, in the 'Mag. Nat. Hist.,' vol. v., 1837. It is characteristic of the Nostochaceæ to increase with great rapidity under peculiar conditions. During June of this year little or no rain fell on the Camden basin for nearly thirty consecutive days, and the sun shone with almost unobstructed power on the still surface of the water. I venture to mention the subject at this meeting, because I am not aware that the plant has been found before in this country, and there are no correct figures of it in the books."

*Effect of Polarized Light on the Body of the Medusæ, and the Crystalline Lens.*—Professor J. Clerk Maxwell, in a paper before the Royal Society, toward the end of December last, says:—"The body of a sea-nettle has all the appearance of a transparent jelly; and at one time I thought that the spontaneous contractions of the living animal might be rendered visible by means of polarized light transmitted through its body. But I found that even a very considerable pressure applied to the sides of the sea-nettle produced no effect on polarized light, and I thus found, what I might have learned by dissection, that the sea-

nettle is not a true jelly, but consists of cells filled with fluid. On the other hand, the crystalline lens of the eye, as Brewster observes, has a strong action on polarized light when strained either by external pressure or by the unequal contraction of its parts as it becomes dry."

*Is the Lichen Solorina bispora a Distinct Species?*—Dr. J. Stirton communicates a note to 'Grevillea' (January, 1874), in which he asserts that it is. He says:—"Since detecting this lichen for the first time in 1871, on Ben Lawers, I have secured it on almost every mountain in Scotland that I have climbed, of a greater elevation than 3000 feet. Accordingly, so far as my experience goes, it is more frequent than *S. saccata*, which is usually found, besides, at much lower elevations—a fact which, in my estimation, ought not to be wholly ignored in the question of specific distinction. In all these instances (four in number) the thecæ are 2-spored, without exception. Occasionally, it is true, a one-spored theca may be seen, where the spore is larger than usual, *viz.* as in one specimen (·1 × ·054 mm.), but, as is well known, especially in the larger spored lichens, such a state is easily accounted for physiologically, although the converse does not hold true. Again, in *S. saccata*, 2-spored thecæ are occasionally though rarely seen in this country, mixed with the 4-spored, where such spores approach in configuration and, to a less extent, in size those of *S. bispora*, but this fact, so far from militating against the specific value of the latter, is, in my opinion, decidedly in its favour, and is merely a counterpart of what (as we have stated) is seen in its own internal organization. In this way is explained what is described by Anzi, and distributed by him from time to time."

## CORRESPONDENCE.

### The Aperture Question.

*To the Editor of the 'Monthly Microscopical Journal.'*

London, *March* 5, 1874.

Sir,—As Col. Woodward now appears to have fallen into the ranks of my opponents, I hope he will excuse me for not offering explanations on this question for his special consideration, as the arbiter by whom the conditions might be set at rest: I have argued that he is wrong in his optical demonstrations, therefore it would be out of character for me to make excuse or apology for an offence that I may at any time repeat; and as his other remarks have been adverted to, I shall confine myself to those on the diagram portion only, leaving others to judge whether the phrases he applies to me, such as "unfounded assumptions" and "dogmatic assertions,"* be altogether

* Vide Col. Woodward's letter, 'M. M. J.' for March last, page 120.

just in a question of simple rigid optical law, where there ought to be no indecision.

It would have been in favour of Col. Woodward's demonstrations if I *had* ignored the fact that the screw-collar adjustment alters the aperture of the object-glass and focal distance. The closing of the lenses *lengthens* this from the anterior surface of front lens, and for an immersion object or one under a very thick cover the lenses are always brought closer. The immersion focus is therefore the outer one, and the dry focus the nearest to the lens: now Col. Woodward has drawn the reverse of this, and in his diagram made the angle for immersion rays the inner one, or *closest* to the lens. This is the first view that may be taken of the demonstration. Much knowledge has been recently acquired of the internal construction of microscope object-glasses; and I should have been glad if Col. Woodward had given us an illustrative figure, having some relationship to a reality with the rays carried to their final destination. The passage of all his rays should have careful consideration, and if I saw no error I could not state that there is one, and trust that I have the candour to admit accuracy. My "mere assumption" had no reference to the construction and position of mythic back lenses, for I challenge the assertion that rays from the two foci that he has shown, can be got through the same lenses of *any combination* and both form a true focus at the back. Considering the optical merits of Col. Woodward's diagram, I ask whether he himself is not liable to the imputation he applies to me, for putting forth such evidence with the absence of all-important back lenses, relative to which he finds fault with me for not knowing the construction and position of? The time must at length arrive when the substantial facts brought to light by this question must be weighed and recorded against mere words, and then the balance may be in my favour.

With reference to other correspondents, this long discussion has on some occasions so inconveniently occupied my time, that in future I must confine myself to the main points of fact and demonstration, and not answer frivolous objections or personalities, that I little care for.

I remain yours truly,

F. H. Wenham.

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## Rev. S. L. Brakey or Mr. Wenham?

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—In the current number of your Journal, Dr. Woodward briefly alludes to the "*more than usually acrimonious language*" of your correspondent, the Rev. S. L. Brakey. Now, sir, this latter gentleman has exhibited a great fondness for dealing in criticism; and as he distinctly says that on the aperture question he knows that "substantially" his "observations must coincide with" Mr. Wenham's

(vide 'M. M. J.,' No. LVI., p. 98), will he try his dialectic on the following passages?

'M. M. J.,' No. XXII., p. 238.

Rev. S. L. BRAKEY: "The only tangible result from these papers [Dr. Pigott's] is the suggestion that the greater brilliancy of water lenses is in *one* case due to the fact that more of the pencil of light is lost in the air lens than in the other from *total* reflexion. This many persons may not have observed, though self-evident when once attention is called to it."

'M. M. J.,' No. XXVII., p. 118.

Mr. WENHAM: ". . . . whether the object is mounted in balsam or not, I challenge Dr. Pigott, or anyone, to get, through the object-glass with the immersion front, a greater angle, or any portion of the extraneous rays that would in the other case [*i. e.* with dry front] be totally reflected, as no object-glass can collect image-forming rays beyond this limit."

If Mr. Brakey is right in saying it is self-evident: where is Mr. Wenham? But if Mr. Brakey knows that his observations must coincide with Mr. Wenham's: then, where is he? I fear in this out-of-the-way place I cannot get this problem solved, I therefore ask your insertion of these few lines in the hope that Mr. Brakey will kindly explain how one may;—

"Confute, change hands, and still confute."

I am, Sir, your obedient servant,

RUSTICUS, jun.

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## DR. URBAN PRITCHARD "ON THE RODS OF THE COCHLEA."

*To the Editor of the 'Monthly Microscopical Journal.'*

KING'S COLLEGE, *March* 8, 1874.

SIR,—In the Address of the President to the Royal Microscopical Society, and in the report published in your last number, the above-named paper was spoken of as if it had been read before the Royal Microscopical Society, whereas it was a communication to the Medical Microscopical Society. The error was not noticed until the Hon. Secretary of the last-named Society politely called attention to it. I am desired to explain that it was quite unintentional, and to request your insertion of this correction.

I am, &c.,

HENRY J. SLACK, Sec. R.M.S.

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## DR. PRITCHARD'S PAPER—MR. MAYALL'S LETTER.

*To the Editor of the 'Monthly Microscopical Journal.'*

16, FITZROY SQUARE, *March* 13, 1874.

DEAR SIR,—My attention has been called by the President of the Medical Microscopical Society to the fact that, in the remarks on Dr. Pritchard's paper on the Cochlea comprised in my Address, I apparently claimed the paper as the property of our Society, by having

omitted to state that it was read before the former Society, a fact which must have been obvious to everyone who read the paper in your Journal: however, as such an interpretation has been put upon my omission, I cannot but regret that it should have been made.

A letter published in page 134 of the last number of your Journal unquestionably conveys a very erroneous impression of my views respecting Dr. R. Pigott's "Aplanatic Searcher," and I trust an equally erroneous version of the observations made on that subject in my Address. From unavoidable circumstances I was obliged to give that portion of my Address *vivâ voce;* but as the printed version consists of the short-hand writer's notes with unimportant amendments, I appeal to my hearers as to whether it may not be taken as a fair representation of my words spoken.

I entirely fail to discover in my printed Address either the *animus* of the letter in question, or a "condemnation" of the "Aplanatic Searcher": as it appears to me, the only legitimate inference to be drawn from my remarks is that I consider its utility "not proven" at present. And so far from having "applied his mathematical skill to investigate the principle of its construction, he had come to the conclusion that its merits were mainly, if not wholly, fictitious," I have expressly stated in my Address that either from the complexity of the conditions of the problem, or from my own want of skill in dealing with them, I had failed in obtaining any result from analysis. Requesting the insertion of these remarks in the forthcoming number of the Journal,

I remain yours faithfully,

CHAS. BROOKE.

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## THE PRESIDENT'S REMARKS ON MR. PILLISCHER.

*To the Editor of the 'Monthly Microscopical Journal.'*

88, NEW BOND STREET, LONDON, *March* 17, 1874.

SIR,—Permit me to correct two mistakes made by our President in his Address read before the Royal Microscopical Society on the 4th February last, in giving his account of the Vienna Exhibition as Scientific Juror.

The President, in the first place, says that I am by birth a "Prussian." This statement is quite incorrect; I am by birth an "Hungarian," and not a Prussian, though I should feel equally proud to belong to the latter nationality; but, having been established in England as a maker of microscopes nearly thirty years, I am perhaps not presuming too much in venturing to express that there was no actual cause for the President to single me out as a Prussian, and might have allowed me the honour to be included in the ranks of English microscope makers.

Our President further says, "and not even in his collection was there a single objective of note or high power."

As this assertion is calculated to do me a great deal of injury, I emphatically and substantially contradict and deny it, and beg most

respectfully to remind the President that on the table in the jurors' room by the side of my microscopes were placed a series of object-glasses, ranging from 4-inch to $\frac{1}{25}$-inch, ready at his disposition for examination, and if they were overlooked by the President in his official capacity as English juror of the only microscopes and object-glasses of English manufacture, it was extreme carelessness on his part, and proves that not even a superficial survey was bestowed by the President upon my object-glasses.

I remain, Sir, your most obedient servant,

M. Pillischer, F.R.M.S.

## PROCEEDINGS OF SOCIETIES.

### Royal Microscopical Society.

King's College, *March* 4, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read by the Secretary, and the thanks of the meeting were voted to the donors.

Mr. Alfred Sanders read a paper entitled "A Contribution towards a Knowledge of the Appendicularia," in which he minutely described the appearance and structure of specimens found at Torquay and Weymouth, and illustrated his remarks by drawings enlarged upon the black-board. The paper will be found printed at p. 141.

A vote of thanks to Mr. Sanders for his communication was moved by the President, and carried unanimously.

Dr. Pigott thought he should just like to ask the reader of the paper whether he had made any observations upon the cilia of these creatures, and also he should like to inquire as to the actual size of the animal.

Mr. Sanders, in reply, said he had mentioned in the paper that the whole of one of the species was ciliated, and that there were abundant cilia upon the other; he had also stated that the size was about half a millimètre, exclusive of the appendage.

Mr. Chas. Stewart inquired if Mr. Sanders had used the binocular or the monocular microscope, as he had found the binocular of the greatest advantage in examining these objects? In looking at some clear Ascidians with a single-barrel instrument, he had himself found it very difficult sometimes to see the heart, but he could do so at once with the binocular. He should also be glad to know if Mr. Sanders had attempted in any way to preserve these delicate forms? A weak solution of picric acid had been recommended; it had the slight disadvantage that it stained them a yellowish colour, but on the whole their forms had been well maintained. They were so very easily destroyed that he should like to know if Mr. Sanders had succeeded

in preserving them. It would be excessively important to ascertain if it was really true that there was a ganglionic cord. It would certainly be very exceptional to find that there was really a chain of gangli lying all along the tail of the animal. Usually in Mollusca they are divided into three portions—one set being near the mouth, another (the pedal) near the ventral portion, and another (splanchnic) near the posterior extremity; but here it would seem as if they had almost a repetition of what was found in much higher organisms, and seemed more like those of insects.

Mr. Sanders said that he had used a monocular microscope for his observations. The use of picric acid seemed a very good suggestion, and he should like very much to know the strength which was recommended for the purpose. He did not see the ganglionic cord; he was not aware of its having been mentioned, so that he did not look particularly for it.

Mr. Chas. Stewart said he did not know exactly the strength of the picric acid; it was usually mixed according to colour, but he would try to ascertain its actual weight. It had been found to answer exceedingly well, with the slight disadvantage of staining.

A paper by Dr. Royston-Pigott, F.R.S., entitled "A Note on the Verification of Structure by the Motion of Compressed Fluid," was read by the Secretary; and was followed by another paper by the same author, "A Note on the President's Remarks on Dr. Pigott's Searcher for Aplanatic Images."

Dr. Pigott then proceeded to give further explanations. He thought he ought also to have added to the paper on the 'Searcher' that the action of the screw-collar was one which was not sufficiently studied in the present day. That in effect it either enlarged or diminished the aperture of the objective. If the lenses were separated it diminished the aperture, and if they were closed it increased it. By closing them they shortened the focal distance and increased the power of the objective. If gentlemen would just take the trouble to do so they would find that by wholly closing the lenses they would give the objective its greatest focal power. It was a matter worth studying, because the whole question of aberration was involved in that simple fact. The whole question turned upon that one thing, the increase or diminution of aperture of the pencil passing through the back lenses by the correction of the screw-collar.* Then there

* Mr. Tolles' $\frac{1}{8}$th, lent me by Mr. Crisp, possesses extremely fine qualities. But if a Kellner eye-piece be used with a *very large field*, objects very much out of the centre are obscured by residuary aberration. Mr. Browning's achromatic eye-pieces, as suggested by Rev. Mr. Webb, cut off so much of the field that only a very small portion is seen at once. The definition of Tolles' glass with such a limited field eye-piece is superb. Bad telescopes are often cured of residuary aberration by contracting the aperture. I wish particularly to call attention to the little instrument called the *aberrameter*, for instantly reducing by a variable stop the aperture of object-glasses (made for me on the iris principle by Messrs. Beck). The aberration is thus instantly controlled by gradually reducing the aperture. That of any simple lens is well known to be in proportion to the square of the breadth of the pencil admitted through it. Of course the angular aperture is reduced by a stop placed at the back of an objective. It is also reduced by separating the lenses of an adjustable screw-collar object-glass. Mr. Tolles' lens

was another proposition, that the aberrations as well as the aperture of the lenses depended on this action. Why was it that, if they approached or receded the front lenses, they corrected the aberration? Why was it? If anyone disputed it, it could be referred to competent judges. If they took the collar and put it to its farthest point, they would find that there was a different diameter to the pencil of rays which passed to the back lenses, and they would also find that there was a different aberration. The aberrations were disturbed by altering the screw-collar, and the cause was that the back lenses were then engaged with a larger or smaller pencil of rays.

With regard to the principle of the searcher, if they got a tube made up of several short pieces, like one which he held in his hand, they could begin with one piece and go on building it up as required. If they shortened the tube the result would be to violently under-correct the objective lenses, and if they now formed the object at 5 in. from the stage instead of 10 they would find the aperture diminished. Now if they put a lens in the tube at different points between the eye-piece and object-glass in a longer tube than usual, they would find that it had different effects according to its position. The actual diameter of a pencil entering the searcher did not exceed $\frac{1}{10}$ of an inch; the consequence was that any ordinary over-corrected lens would work with it. He found the back lenses of Powell and Lealand's inch object-glass made with three sets of lenses formed a very good searcher.

The President thought he should like to ask Dr. Pigott a question as to his "searcher." In his communication he spoke of the effect of altering the adjustment screw, and described it as altering the angular aperture. That was unquestionably so, but though it was a feature it was not the only object to be attained. He thought that the mode in which the relations were altered by the progression or recession of the front lenses from the combination was laid down by the late Andrew Ross with very great clearness some years ago, and he should like to ask Dr. Pigott whether the action of the aplanatic searcher could be laid down diagrammatically in the same clear manner?

Dr. Pigott, in reply, said that if he was desired to draw a diagram he should be very pleased to do so. He then drew two upon the black-board and explained them whilst they were in progress, showing changes in aberration with variation of the position of the searcher

has a much larger angular aperture when the lenses are closed up than when fully open, and the aberrations are proportionately changed. Probably one-hundredth of the turn of its collar does not change the linear aberration more than the fifty-thousandth part of an inch; nor the angular aperture more than a minute proportion of a degree. But he states in one of his contributions the exact amount of change (overlooked in England) in the angular aperture for opening or closing the lenses fully. *Using a thin cover micrometer, I have just ascertained that Tolles' glass magnifies with 2-inch eye-piece as follows:—350 diameters with fully open lenses, 470 where they are fully closed.* Dr. Woodward (November Journal, p. 214) states that a Tolles' $\frac{1}{10}$th has a balsam angle of 65° at the open point, and when the screw-collar is fully closed it becomes 87°. The change of aperture by change of screw-collar has not apparently received sufficient attention in this country.—G. W. R. P.

between the eye-piece and object-glass. This pursuit he assured the meeting was an arduous one, there was the focussing, then the length of tube, the position of the searcher, and the adjustment of the screw-collar, and they knew how many changes could be got out of four bells, but here were four quantities, each capable in itself of very great variation, so that it might be judged how much was involved in making these investigations. One of the most pleasing results of his labours was found whilst examining one of Mr. Slack's silica films. In working one day upon one of these slides he found that in particular positions of the screw-collar of the searcher and of eye-piece those cracks which were so red before now became beautifully black, so that the searcher corrects chromatic aberration also. The subject was so intricate and difficult that he should recommend no one to take up the searcher, unless they were prepared to meet difficulties at every point. He hoped soon to be able to show a combination which gave to a quarter four times the usual power under the use of a low eye-piece instead of an uncomfortable deep one. Messrs. Powell and Lealand might very likely be disgusted to hear that he often used their $\frac{1}{50}$-inch at five inches distance, instead of ten; of course, as already stated, it quite * under-corrected the lens, but he got a brilliancy which astonished him. If any gentleman would take the trouble of getting a tube made like the one he held in his hand, and would commence with the first piece and go on building it up as he had described, they would see such a beautiful series of effects as would well repay them the cost, 10*s.*, expended upon it. He had never found that an objective ever worked really well with any other covering glass than that for which it was originally made. Could anybody tell him what "covering glass" was made of? He thought it so important that he had constructed an elaborate instrument (a drawing of which he exhibited) to measure its refraction.

Mr. Wenham said he believed it was a light flint.

Dr. Pigott: Yes, it was the lightest flint glass that could be found. He then drew attention to a drawing of a new refractometer on paper, showing a method of obtaining the refractive index of thin plates of glass, and he had found that its refractive index was 1·555 for mean rays.

The President thought that as a means of obtaining the refractive index this plan was so important that he hoped Dr. Pigott would make it the subject of a paper.

Dr. Pigott explained the principle upon which the instrument was based. He said when a piece of glass was placed over any object it caused the image to rise, and this effect being proportionate to the refractive index of the glass it was only required to measure the distances with and without the covering glass, and the refractive index could then be obtained with great exactness.

Mr. F. H. Wenham said he should like just to say a word or two upon three points touched upon by Dr. Pigott. First, with respect to the Podura scale-markings, the subject had been so controversial that it was perhaps hardly proper to speak of it as one in which this

* Compensated, of course, by separating the lenses by the screw-collar.

Society could be involved. It is well known that from the first he held the idea that these so-called spurious spines were a reality, and he was now able to show a specimen in which one of the "spurious" (?) spines was a detached body, which had been fairly dissected and cut from the scale. With respect to the correction of the object-glasses for aberrations of covering glass, or differences of conjugate foci, he agreed with Dr. Pigott that a correction might be obtained in the way described. If they began with a short length of tube, for every inch they added, there would be a proportionate alteration required in the adjusting collar, and therefore a position might be found for the eye-piece that would correct aberrations apparent in another position. But he disagreed with the statement that the aberrations which were effected by an adjusting collar were in any way due to an extra number of rays introduced, and caused by mere increase of aperture only. These aberrations were not due to exterior rays, and it was quite certain that in any case, whether the aperture were large or small, the same series of the effects of chromatic aberrations would be found to occur. Taking the light point from a very minute mercury globule in the true focus of the object-glass, which is the real point from which all the corrections must be made, and viewing the sections of the light cone both within and without that focus, the indications would be the same, whether the aperture were made larger or smaller by stops; that is to say, in either case there would be under-correction by approximating the lenses, and over-correction by separating them.

Dr. Pigott said he would not trouble the meeting at that late hour with any lengthened remarks. He was sorry that Mr. Wenham did not agree with the simple principle he had laid down. He believed it was quite correct. He knew of no other form in which to express it than as the coefficient of $y$ square (as given in treatises on Optics).

The President suggested that it was only *spherical* aberration to which Dr. Pigott's remarks applied.

Dr. Pigott said he was now alluding to spherical aberration. He was prepared to maintain that the linear aperture of the pencil, passing through the object-glass, regulated the spherical aberration. If they moved the screw-collar, by so doing they altered the linear aperture of the pencil; therefore they altered the aberration which varies as the square of the linear aperture.

The President said this was perfectly true, but it should be borne in mind that the alteration of the screw-collar did another thing, it also altered the position of the lenses; alteration of the aberration depended also upon the passage of rays through the combination.

Dr. Pigott admitted that he was not tightly bound to the angular aperture as the only explanation, though he thought it might be useful to explain how the alteration in aberration was really effected by a movement of the screw-collar.

Mr. Wenham said that his remarks were based not upon any mathematical formula or demonstration, but were simply taken upon the usual practical method of measurement, with an artificial star viewed within and without the focus proper, as the indications from

this position were the only ones from which an object-glass could be corrected and constructed. As regarded the question of angular aperture, he felt sure that there had been much mistake about it. No doubt, as he was one of the parties in the dispute, it would not do for him to pass judgment by measuring the glasses of different makers in contradiction to their own lists, but it appeared to him that many mistakes had been made because a great deal of false light entered at extreme incidences. This could be easily shown, and he was sure that oftentimes a mistaken increase of aperture was due to such false light and not caused at all by image-forming rays. By placing a minute stop, with its front plane, exactly in the focal point of the object-glass, this might be demonstrated, as such a stop would prevent the entrance of false light beyond the range due to true aperture, and possibly from this cause the same glasses had sometimes been found to give such widely different measurements in a short range. In one or two instances he had already found that with this safeguard attached, the entire range given by the maker, between "covered" and "uncovered," did not show a difference in the angle of aperture, or but a very slight one. By due investigation such a condition could probably be accounted for.

Mr. Slack supposed there was a real change, but that the false light interfered with it.

Mr. Wenham assented to this, and said that the question was of so much importance that he thought of making it the subject of a communication to the Society, and would then bring an instrument by which it could readily be tested, and thus furnish a demonstration at the next meeting.

The Secretary thought that as Mr. Stephenson had worked with Dr. Pigott's instrument, perhaps he might be able to give them his opinion of it.

Mr. Stephenson said he was afraid he was not in a position to offer any observations. He had certainly used the searcher, and he quite agreed with what Dr. Pigott said about its being very difficult to use. If required merely to increase magnifying power, it would be found much more useful than any eye-piecing.

Mr. Slack would only say that he hoped some Fellows of the Society would pay attention to Dr. Pigott's method of testing objectives; he did not presume to give an absolute opinion upon it, but having seen it in operation for many hours on various glasses, he believed it would enable them to make a reliable *quantitative* comparison. It was, he thought, a matter of much importance, especially to those who made good objectives.

Dr. Pigott said he had come up from Reading to attend that meeting, in order to thank the President for the handsome manner in which he had referred to him in his Annual Address, but he had seen with great surprise a letter in the last number of the Journal, in which it was stated that the President, in his Address, said the searcher was a wholly fictitious thing upon its merits.

The President, interposing, said that his remarks on that occasion

were in print, and therefore it would be only necessary to refer to them to show that he had said nothing of the kind.

The meeting was then adjourned to April 1st.

Donations to the Library since February 4, 1874:—

| | From |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| The Lens. Vol. 2, No. 4 | *Editor.* |
| Annual Report of the Smithsonian Institution, 1871 | *Institution.* |
| Metamorphoses of Man and the Lower Animals. By A. de Quatrefages. Translated by Dr. Lawson | *Dr. Lawson.* |
| Journal of the Linnean Society. No. 57 | *Society.* |
| Quarterly Journal of the Geological Society, No. 117 | *Ditto.* |
| Bulletin de la Société Botanique de France. Two Parts | *Ditto.* |

Dr. J. J. Drysdale and William Payne, Esq., were elected Fellows of the Society.

WALTER W. REEVES,
*Assist.-Secretary.*

## MEDICAL MICROSCOPICAL SOCIETY.

The first annual meeting of this Society was held Friday, January 16, at 8 P.M., at the Royal Westminster Ophthalmic Hospital, Jabez Hogg, Esq., President, in the chair.

The minutes of the previous meeting having been read and confirmed, the Secretary proceeded to read the Report of the Committee. From this it appeared that the Society, though only one year old, was in a flourishing condition. During the year 129 members had joined it, and sixteen papers had been read, each of them being followed by a lively discussion, and at no meeting had there been any lack of specimens for exhibition.

The Treasurer's Report was also satisfactory. Ninety-four of the members had paid their subscriptions, amounting to 47*l.* There had been spent for the Society 36*l.* 10*s.* 3*d.*; thus leaving a balance of 10*l.* 9*s.* 9*d.* Besides this, 35 members still owed their subscriptions, which would make 17*l.* 10*s.* more to be added to the balance.

The following gentlemen were elected officers for the ensuing year:—*President*, Mr. Jabez Hogg; *Vice-Presidents*, Mr. W. B. Kesteven, and Drs. H. Lawson, J. F. Payne, W. Rutherford; *Treasurer*, Mr. T. C. White; *Hon. Secretaries*, Messrs. C. H. Golding Bird and J. W. Groves; *Committee*, Drs. M. Bruce, E. C. Baber, U. Pritchard, W. S. Greenfield, W. H. Allchin, J. Matthews, and Messrs. H. Power, F. T. Paul, J. Needham, G. M. Giles, S. Coupland, E. A. Schäfer.

The President then read his Address, after which votes of thanks were accorded to the various officers, and the proceedings terminated.

## Reading Microscopical Society.*

Jan. 6, 1874.—Capt. Lang, in a short paper entitled "A Useful Hint for Mounters," said, "Persons who select and arrange diatoms or pursue any minute work under the microscope, require and have often to improvise implements adapted for special purposes, as hairs, from various animals, whipped on to delicate handles. There is none that I know of better than the hair of the badger, or fine camel-hair or sable brushes. But I have often found with diatoms that some obstinate valve in a roughly-spread dip refuses to be picked up, or, if moved, is suddenly flipped out of the field of view and lost. On one occasion, after having vainly tried to lift up a rare diatom with hair or brush, it occurred to me that painters occasionally make use of the fine feathers, one of which is to be found on the extreme end of the carpal joint of each wing, of the woodcock. On trying one of these I found I had nearly got what I wanted, that it was an excellent implement for microscopical work in general, though scarcely sufficiently delicate for the selection of diatoms. It struck me at once that there might be other birds belonging to the same order that might answer my purpose better. The feather of the snipe, as that of a smaller bird, was tried, but was found not to be so sharply pointed. That of the golden plover was then obtained, and has answered all my requirements, combining fineness, stiffness, and elasticity. With it the most refractory diatom may be lifted, transferred, turned, cleaned, and placed in position; indeed, by its means an entire frustule may be divided into its two component valves, which are in many cases dissimilar, and thus an instructive slide of a whole frustule and its two separate valves can be prepared. It may be as well to remark that these feathers vary considerably, some being much more finely pointed than others, so that it is worth mounting at least half-a-dozen of them, as some will be found better adapted for special purposes than others. Of course their use is not confined to the selection of diatoms, as they are equally adapted for other microscopical manipulation, as in the preparation of entomological subjects."

## Microscopical Society of Victoria, New South Wales.

[We give this report in full, because of its great interest, and also because it is the opening address.—Ed. 'M. M. J.']

A short time since a small number of microscopists met together in Melbourne, and decided, with a view to securing the advantages of systematic co-operation, to form the above Society. That fact having obtained publicity, the promoters of the movement received several communications which showed that they had more fellow-microscopists in Melbourne than they expected. Already about thirty gentlemen have joined the new Society, and it is probable that residents in other colonies will be admitted to it as corresponding members. There are two grades of membership—members and associates. The former pay

* Report supplied by Mr. B. J. Austin.

an entrance-fee of one guinea and an annual subscription of the same amount, and the associates pay merely the guinea subscription. Country residents are admitted on payment of the above entrance-fee, and 10*s.* 6*d.* annually. The first General Meeting of the Society was held in the Royal Society's Hall, October 10th, 1873. About forty gentlemen were present, the President (Mr. W. H. Archer) in the chair. A rich and varied collection of microscopes and objects was shown by members of the Society, during the evening. These exhibits were examined with interest, the exhibitors willingly giving information to all inquirers.

The President read the following address:—We meet to-night for the purpose of inaugurating the Microscopical Society of Victoria. It will of necessity consist of two classes of persons, namely, skilled workers, who are called members, and students and amateurs, who are called associates. The first class, it is expected, will be constantly recruited from the second, and so render skilled working our fundamental characteristic. We have in Victoria microscopists who are possessors of good instruments, and who know thoroughly how to use them. The establishment of this Society, it is hoped, will induce most of these gentlemen to co-operate, sooner or later, with one another in a methodical way, to the enlargement of the bounds of known truth by means of the microscope. For though at intervals certain very valuable special professional work has been accomplished in this city and elsewhere, yet so far as published results are concerned, I believe I am justified in declaring that at this moment not only Victoria, but Australia generally, is, microscopically speaking, almost altogether an unknown land. It is fitting that I should here allude to those Victorians who have given the results of their microscopical labours to the world. First on this honourable list I place Baron von Mueller. This eminent scientific botanist, in the course of thirty-four years of independent labour in the field of photography, with some sort or other of microscopic instrument as his daily companion, has scrutinized about 30,000 species of plants. In the comparison of the plants of the Australian continent with those of other parts of the globe, the new definition of orders, genera, and species, from hitherto unknown material, necessitated frequent analytic dissections, and microscopic examinations of minute organs, involving an amazing amount of patient persevering application. But notwithstanding all the indefatigable efforts of this able investigator, the simple impossibility for one man to do everything, even in his own sphere, has compelled him to pass by vast untrodden fields of exploration in the shape of cryptogamic botany, wherein any number of aspirants for microscopical fame can therefore still find ample opportunity for winning the highest renown. Next there is Professor M'Coy, who made his mark with the microscope at home by first establishing, contrary to the received opinion of the time among all microscopical anatomists, that the polished part of the surface of fish teeth is not enamel, but a modification of dentine (ganoin). He has likewise worked at recent and fossil sponges, and a little at polyzoa and foraminiferæ since he came here, but he cannot get time for mounting or preparing objects for

investigation himself, and has, therefore, had of late to depend on microscopical friends. It would be indeed gratifying to all concerned if this Society could even in the smallest degree, at any time, facilitate the development of the learned professor's illustrated series of *Decades*, of which many beautiful plates connected with the first part have been lying unissued for several years. Speaking of the *Decades*, puts me in mind of Mr. McGillivray, whose love for natural history is an hereditary possession. Some years ago he helped Professor M'Coy in determining certain Victorian polyzoa, and also contributed three papers on those zoophytes to the Transactions of the Philosophical Institute and Royal Society of Melbourne. He also described the first known Australian fresh-water polyp, the *Plumatella Aplinii*, which he named after Mr. Aplin, the finder. I trust sincerely that this accomplished naturalist, in spite of his arduous labours as a professional man, will not altogether give up microscopical manipulation. A prior contributor, however, to the pages of Victorian microscopy exists in Mr. Sidney Gibbons, who is one of the earliest pioneers in the effort to make the microscope a popular instrument in this country. In 1852 he commenced inquiries into the adulterations of food, which he has continued ever since. In 1855 he read a paper before the Victorian Institute on microscopic investigation, and some minor details of manipulation. In this paper he described, among other things, the invention of a cutting machine, which eventually found its way into the text-books. In 1857, Mr. Gibbons wrote two popular microscopical papers for the 'Journal of Australasia.' In 1858 he started a new method of micrometry. In 1866 he was engaged as an expert on behalf of the Government to bear testimony in a murder case as to the existence of stains of human blood. He was at the time exposed to some obloquy for his unhesitating assertion that he could safely swear to the character of those stains under the conditions given; but whatever room may still exist for discussing the validity of his reasons, it must be a gratifying circumstance for him to have learnt from subsequent revelations that his conclusion as to the fact itself was certainly right. In 1868 he was engaged by the City of Melbourne Corporation to report on the sewerage of that city, with particular reference to the system of cesspit filtration, and to percolation, which involved considerable microscopical work. He also reported during the present year on waters sent him for chemical and microscopical analysis by the Beechworth Shire Council. In August and September, 1872, he published two papers in the 'Australian Mechanic,' headed "The Yan Yean under the Microscope," in which he defends the comparative purity of our general water supply. It is right also to mention that Mr. Gibbons, conjointly with the Rev. Dr. Bleasdale, founded some years ago a society called the Microscopical Society of Victoria. Its name, unhappily, was symbolical of its fate, for by degrees it became truly microscopic, and at last resolved itself into sheer invisibility. Dr. Bleasdale, too, as well as Mr. Gibbons, was, in days gone by, very zealous in his endeavours to promote the advancement of microscopy, and was associated with the Government analytical chemist, the late Dr. Macadam, and other medical men, in

many delicate investigations. I am not, however, aware that any traces of the rev. doctor's microscopical labours exist in print. Mr. Gibbons has also steadily endeavoured to popularize the microscope by lecturing and teaching thereon. He has likewise worked at micro-photography. And under this head I must not omit naming that able photographer, Mr. Noone, of the photographic branch in the Crown Lands Department. And now I come to that most skilful and veteran microscopist, Thomas Shearman Ralph. That gentleman, in the year 1857, exhibited at a conversazione of the Royal Society some fossil diatoms found by him in a railway embankment on the South Yarra swamp, and at a subsequent meeting of that body a paper on these Diatomaceæ was read by the late Dr. Coates. The principal species shown was one closely resembling *Eupodiscus Ralfsii*, named after Ralfs, the author of British Desmidiaceæ. There were also some Campolydisci among upwards of fifty different species of various genera not then properly determined. From this embankment many microscopists have been supplied in England and elsewhere. Subsequently, in December, 1865, Mr. Ralph, through the Medical Society of Victoria, made known his microscopical observations and experiments on the effects of prussic acid on the animal economy, showing that the iron in the blood is acted upon by the exhibition of prussic acid, and that prussian blue is formed and starchy bodies produced. A paper by Professor Halford on the action of magenta on the blood, published in 1866, stimulated Mr. Ralph to make further experiments as to the effects of various chemical agents on that substance, and he soon after gave the results of his observations on the effect of magenta, cupriate of ammonia, &c., and maintained that the red corpuscles of the blood are spherical and not discoid while circulating. In June, 1869, Professor Halford having read at the Medical Society a paper on the condition of the blood after death from snake bite, in which he pointed out the altered condition of the corpuscles, Mr. Ralph followed up the same subject in December of that year. In 1871 Mr. Ralph produced further observations and experiments with the microscope on the chemical effects of chloral-hydrate, chloroform, prussic acid, and other agents in the blood; and in August, 1872, he read a paper at the Medical Society on the existence of minute bodies in the blood other than the white and red corpuscles. This had especial reference to scarlatinal poisoning of the blood. Professor Halford, in addition to his labours already mentioned, has proved still further his experimental ability and assiduity in research by the production of a paper entitled "Experiments and Observations on Absorption," in which the microscope played a prominent part. With the mention of Mr. Watts, who contributed to the Transactions of the Philosophical Institute of Victoria a list of some Victorian Desmidiaceæ, and drew attention to certain fossil polyzoa observed by him; also of the Rev. Julian E. Tenison Woods, who, in a paper read in Melbourne, described some fossil polyzoa found at Mount Gambier; and of Mr. Maplestone, who has recently made some careful notes on Victorian mollusca and their palates, which were published in the Transactions of the Royal Microscopical Society of London in 1872.

[See the 'M. M. J.' for August, 1872, which contains a series of plates illustrative of Mr. Maplestone's important inquiry.] I believe I have nearly, if not quite, exhausted the list of those who have published the results of microscopical researches made in this country. Purely private labours do not come within the scope of my address this evening, and therefore my own microscopical work, with that of Mr. Ellery, the Government astronomer, and some others similarly situated to him and myself, must remain for the present in the background.

Now, while this brief summary gives, I think, ample proof that we have skilled workers amongst us, does it not, gentlemen, most strikingly show how much has yet to be done in Victoria, which, in common with all Australia, I have ventured to designate, for our purposes as a Microscopical Society, a true *terra incognita*? Taking this for granted, it becomes a question for your committee to devise the simplest practical means for lifting the veil of our ignorance. Steady methodical action is necessary. What direction, then, shall it take? Permit me to suggest—First, that the committee, as skilled microscopists, should agree upon each individual member taking up definite lines of microscopic inquiry, such lines being more especially chosen which have a practical interest for Australians; secondly, that it be understood the worker in each line shall receive the hearty co-operation of all the others, as opportunity offers at periodical meetings; and, thirdly, that such workers shall aid and utilize the labours of all willing students and amateurs throughout the colony. It is evident, gentlemen, that by marking out separate lines of inquiry an economical division of labour will be ensured, and every goal more readily reached; while mutual co-operation, by way of frank suggestion, manipulative help, and friendly aid in the shape of exchange and gift of specimens, will multiply rather than lessen the laurels to be gained by each worker in his own walk. And now, if the Society were to consist solely of trained microscopists, we might content ourselves here by the expression of our determination to commence a new career to-night, and start at once, like brothers, hand in hand. But there is a daily increasing class of well-educated persons, as well as students and amateurs, which it is desirable to include within our circle of action—a class that I verily believe can be made, with some degree of painstaking, a most effective means of enlarging the sphere of physical science in this country. The country wants observers. In every locality there are novel things to be found, and those only who are on the spot can find them. In order, then, to enlist an intelligent corps of such observers, I beg to suggest that—1st. Our committee draw up a series of tables of objects to be looked for, and things to be done, in order that a calendar of nature may be framed, in relation to microscopical investigation throughout Victoria, for every month in the year. Such tables would include directions relating to infusorial, cryptogamic, and insect life, as located in our gardens, and on the wayside, in our lakes and rivers, in the wild bush, and on the sea-shore. 2nd. I would suggest that agricultural and horticultural societies, and other local bodies, also

teachers of state and other schools, and desirable private persons, be, at their request, supplied with such tables, so as to enable them to co-operate with the Microscopical Society as collectors of objects; this anticipated collection, it being understood, involving no trouble or responsibility beyond the voluntary transmission from time to time of any object of interest, with a short memorandum of a few prescribed particulars duly attached. And lastly, that on any one of such corps of observers becoming an associate of the Society, he shall receive from the skilled members all advice and assistance, by correspondence or otherwise, as may be found most convenient, to enable him to make choice of such microscopical instruments and apparatus as may be most suited to his purposes; and, further, to aid him as far as possible to become in every way a thoroughly competent microscopist. Many of us know, from personal experience, how easy it is for beginners in microscopy to throw away both time and money for want of a little able and disinterested advice. The assistance that can be effected by the Society under this head will be in the long run, I am sure, highly valued. You see here to-night a wide range of specimens of English instrumental skill, from the luxurious first-class productions of Ross, Powell and Lealand, and Smith and Beck, to the economical but useful work of Baker, Collins, and Field. If time were allowed, I should like to dilate on the respective merits of the instruments with which I am most familiar, in respect to various methods and objects of inquiry; but this will follow probably hereafter to sub-committees of our Society. I may perhaps, however, be permitted, in passing, to draw your attention to the specimens on the table of Smith and Beck's educational, and to their popular binocular, microscope; and, likewise, to Collins's binocular, all of which the Minister of Instruction has kindly lent us for the evening. These were among the instruments introduced by me for educational purposes during the ten years I had the honour of a seat at the Board of Education. One of the most interesting and practically useful objects for occasional investigation and discussion at our meetings will be the accurate determination of the real value to working microscopists of the various stands, objectives, and accessory apparatus so prodigally developed by makers in the mother-country. But indeed we should not confine ourselves to the results of English industry. Hartnack, of Paris, appears to be leading the way on the Continent to greatly improved optical work; and Tolles, Spencer, and Wales are said to be doing marvels in America. I hope to see the day when we shall have choice proofs of what the whole microscopical world can produce collected around us, and carefully tested by our own eyes and hands in our own Hall in Melbourne. One other thing, gentlemen, you as well as I should be rejoiced to see, and that is a really useful microscope of Victorian manufacture. At present, the idea is naturally provocative of a smile, but I cling to the belief that not only among the adult immigrant population, but even among our native-born youth, we shall some day find thorough mechanicians, who will emulate the marvellous skill and persistent energy of their forefathers. Look at the triumphs of the American microscope

makers. Their conquests are literally but of yesterday and of to-day. A generation ago microscopes were a rarity in America. In the year 1840, when the United States' exploring expedition to the South Seas, under Commander Wilkes, was fitting out, it was thought necessary to have a microscope. The various makers of scientific and philosophic instruments were applied to, but none of them could furnish the expedition with the thing desired. In this dilemma a private individual was appealed to, and an instrument thus finally obtained, in the shape of an inferior French microscope. How, then, did the present flourishing state of affairs come about? Simply by the genius of a self-taught man. He was a backwoodsman, and had pored over an old cyclopædia, and turned the optical knowledge contained therein, as far as in him lay, to sound practical account. At the age of twelve years he made his first lens. One day he happened to be shown a microscope constructed by Chevalier, of Paris, and the thought struck him that he would try to make a similar instrument. He succeeded, and his glasses were able to resolve a test which similar objectives of the first English opticians had hitherto failed to define. His name was Charles Spencer. And now his pupil Tolles, and Wales, a pupil of Smith and Beck, with Gronow, Zentmayer, and others, form a galaxy of American mathematical instrument talent that appears from recent accounts to be holding its own against the whole of the Old World. Is there not here a ground for the hope I expressed a little while ago? Surely after this example of Spencer, the young backwoodsman, many here present may live to see the day when a finished microscope shall be presented to their delighted gaze by the hands of an Australian townsman, at least, if not by an Australian bushman.

Reverting to the different classes of colonists from whom we hope to elect intelligent associates in aid of our microscopical inquiries, I wish to mention, in explanation of my suggestion that agriculturists should unite with us in securing natural-history objects, that I have not overlooked the fact of a Government Department of Agriculture having been formed recently under a Minister of the Crown. In the course of time, when the scope and functions of that department shall have assumed a definite limit, its sphere of operation will no doubt comprise the labours of a Government microscopist. No such officer has, however, yet been appointed; and in the meanwhile specimens of material, whether forwarded to us direct by agriculturists or through the Minister of Agriculture, might here undergo the microscopic investigation required. Any reasonable expense incurred in such researches would, I understand, be cheerfully borne by the Government. I am authorized to say this by the hon. the Minister of Agriculture, Mr. Casey, and the result of these researches, with accurate illustrative drawings, would doubtless find full publicity in any publications of the department, such as its annual report. The Secretary of Agriculture, Mr. Wallis, has handed me for presentation to you this evening a coloured drawing of a new and very destructive vegetable parasite, with some specimens of the plant itself, which has appeared recently among the rye-grass in the neighbourhood of Ballarat. Baron von

Mueller describes it has a Clavaria. It will be important to learn if it has made its appearance in any other part of the colony, and under what circumstance as to crop, soil, and locality.

And now with a view of encouraging microscopical students and amateurs who, perchance, may be thinking that anything they can ever do in the way of natural-history research must necessarily be of small account, I would beg permission to cite one or two examples which, in my humble judgment, should henceforth give them a stout heart for future zealous exertion. Who would expect to find an original, independent, and successful marine naturalist in a bustling London merchant? And yet, somewhat a little over a hundred years ago, one John Ellis achieved in that character, in the great metropolis, an imperishable renown. He was fond of amusing himself in making imitations of landscapes by the curious and skilful disposition of delicate sea-weeds and corallines on paper; and it was this amusement that directed his inquiries into the nature of the latter, for, says Dr. Johnston, attracted by their beauty and neatness, he was induced to examine them minutely with the microscope, by aid of which he immediately perceived that they differed not less from each other in respect to their form than they did in regard to their texture; and that in many of them this texture was such as seemed to indicate their being more of an animal than of a vegetable nature. These suspicions, as he modestly termed them, were communicated to the Royal Society of London in June, 1752. And, encouraged by some of the members, he prosecuted this inquiry, continues Dr. Johnston, with such ardour and care and sagacity, that in August of the same year he had fully convinced himself that these apparent plants were animals, in their proper skins or cases, not locomotive, but fixed on oysters, mussels, and sea-weeds. And in 1755 he published his famous essay towards a natural history of the corallines and other marine productions of the like kind found on the coast of Great Britain, a work, says the historian of the British Zoophytes, so complete and accurate that it remains an unscared monument of his well-earned reputation as a philosophical inquirer. It is even to this day the principal source of our knowledge in this department of natural history. The Royal Society eventually adjudged to Ellis its highest honour, in the shape of the Copley Medal, for his most ingenious and accurate investigation, which forms an epoch in the history of natural science. So much for the city man of business. Now, for the emulation of teachers of youth, I will cite the example of Abraham Trembley. He also made himself world-famous in the middle of the last century by the production of a most remarkable quarto volume of several hundred pages, a copy of which I hold in my hand. It is in French, and consists wholly of the life history of fresh-water polyps. The author was living about the year 1740 at Sorglviet, at the château of the Count de Bentinck, with two pupils. He one day happened to observe some minute creatures in the waters of a pond in the neighbourhood, and his curiosity as to their nature was excited. He cut one of them in two, and to his astonishment he found that the separated parts became each an individual whole, and that thus two creatures then existed where there had been only one

before. He proceeded to cut another into numerous morsels, and still each of these morsels became a separate living thing. In his book he gives an engraving of himself and his two pupils in the chamber wherein he carried on these remarkable experiments; and he alludes to those pupils as sharing in his hunts after the polyps, and to the charms which the contemplation of nature has even for the very young. It is certainly doubtful whether any two lads ever had the chance of seeing such marvellous work in the whole wide world before. But more astonishing things were yet to come. He next cut a polyp longitudinally, commencing by the head towards the end of the tail, so that it formed two bodies, two heads, and a tail. The separated portions being kept apart, then developed so that each part consisted of a head and body. After feeding this polyp with the two heads by its two mouths, he cut each of these parts longitudinally as before, and in a short time there were four heads. At last by a similar process he produced seven heads. One would have thought that Trembley by this time would have been glutted with marvels. When Hercules struck off the heads of the Lernean Hydra that dwelt in a swamp, two heads grew forth each time in place of the one decapitated, but when by a bold stroke Trembley cut off the seven heads of the polyp I have just described, not only did seven new heads appear after some days, but the very seven heads that were cut off took food and became perfect animals! And, beyond all this, Trembley, by the most exquisite delicacy of manipulation, turned these beings inside out, as one would turn a glove, and yet these miraculous creatures ate and lived and thrived as if to the manner born. Surely mortal man never engaged in a more striking biological experiment than this. Well might all Europe ring with the exploit, and ambassadors hasten to remit specimens of the wondrous hydra from nation to nation. And this all resulted from a modest teacher of youth, having clear eyes, and with good set purpose honestly using them. On thoroughly looking through this original edition of Trembley, you will perceive that the last eight plates were engraved by his friend M. Lyonnet, and there is a pleasant history attached to these. The author in alluding to them says that no doubt the reader will be surprised that the said M. Lyonnet had not achieved all the illustrations, but the truth was that at the time M. Trembley was producing the first part of his work M. Lyonnet had never touched a tool or seen an engraver at work. And our author goes on to describe in what manner, and how in an incredibly short space of time, his friend acquired a most singular graving skill. So that in so far as Trembley was an amateur zoophytist, Lyonnet was an amateur artist on copper or steel. Well might Trembley say, as he looked on these exquisitely drawn figures, that they were in one sense as great a wonder as were the prodigies they represent. Take it altogether, this book, both in composition and illustration, is most charming in its simplicity, accuracy, and felicity. It is often cited, but I do not know that it has ever been translated as a whole, or even in parts, to any considerable extent. Such a book is to my mind enough to fill with zeal anyone who has a spark of the love of natural history in his bosom. If time permitted I should wish to encourage further both skilled workers and

students by dilating upon one other instance at least of extraordinary success with the microscope. I allude to that of the illustrious Pasteur. You are all aware that the culture of the silkworm has for many years been a prominent industry in France. In 1853 the revenue from silk produce was one hundred and thirty millions of francs. In 1865 it was reduced almost to nothing. The whole country was aghast at the terrible infliction. All efforts, scientific as well as others, had failed. But the great French chemist, Dumas, one day happily thought of enlisting the services of his friend, colleague, and pupil, Pasteur. Now Pasteur had never seen a silkworm, and urged his inexperience. Five hundred thousand francs had been offered by the Minister of Agriculture for an infallible remedy. M. Cornalia, in 1860, had declared that the pharmacopœia of the silkworm is now as complicated as that of man. Gases, liquids, and solids, he cried, have been laid under contribution. From chlorine to sulphuric acid, from nitric acid to rum, from sugar to sulphate of quinine—all has been invoked on behalf of the unhappy insect. At this stage Pasteur consented to commence his inquiries. He made his first communication to the Academy of Sciences in September, 1865. It was received with an attitude of hostility. He was only a chemist, some said. What did he know of biology? But to make a long, though very interesting, story short, Pasteur soon showed what a man with good eyes, good glasses, and steady *nous* can do for the welfare of his fellow-men. Both Professors Huxley and Tyndall have eulogized Pasteur's work, 'Sur la Maladie des Vers à Soie,' wherein the author describes in detail his method of securing healthy eggs, which process is nothing less, says Professor Tyndall, than a mode of restoring to France her ancient prosperity in silk husbandry.

In conclusion, I beg most earnestly to urge upon both the members and associates of this Society, that in their efforts to contribute to the existing treasury of science in the shape of methodized facts, they must not expect to do great things all of a sudden. Let us but commence to-night quietly and modestly, with the resolve to seek for and do patiently whatever work we may find waiting for us, and rely upon it, gentlemen, our new Society, young as it is, may equally with its elder sister the University, utter the confident prediction that it will unceasingly grow in the praise of posterity.

"Usque ego postera
Crescam laude recens."

At intervals other addresses on special subjects were delivered.

Mr. Ralph gave a short statement of experiments he had made with the view of ascertaining the combined action of prussic acid and ammonia on vegetable tissues. He had discovered that the sap in the stem of a vine contained quantities of iron, but at certain periods only. When the fruit began to form, iron disappeared from the sap, thus indicating that the plant underwent a chemical change. It was probable that the iron went into the fruit, but of that he was not sure.

Mr. Sidney Gibbons demonstrated with the aid of drawings how

the character of water can be determined by its occupants. Some animalculæ, he said, could only exist in pure and sweet water. When placed in impure water they died, and gave way to others of a lower organism. There was, in fact, a descending scale—the more impure the water, the lower the organisms in it. Amongst the usual occupants of pure water were the four-horned cyclops and the desmid. These could not live in impure water, but he had found them in the Yan Yean. About this time of the year Yan Yean water assumed a slightly opalescent greenish tint. That was caused by shoals of desmids, which multiplied in the spring. He felt quite justified, after repeated tests, in vindicating the purity of the water supply of Melbourne.

Dr. Wigg alluded to the subject of foraminifera, or minute shells. Living specimens of this family had, he said, been found on our coast as large as fossil specimens found in other parts of the world, where such large ones were now extinct.

Subjoined is a list of some of the exhibits, a glance at which was sufficient to show that the microscope has opened up to those who have it under their command a new world:—The president showed a range of Ross's objectives, from the 2-inch to the $\frac{1}{12}$-inch. The stands he employed were Smith and Beck's first-class smaller, Smith and Beck's educational, Smith and Beck's popular binocular, Collins' Harley's binocular, Beale's clinical microscope, and a new dissecting microscope. The objects shown by him were sections of Australian woods, Australian polyzoa, hair and wool of Australian animals, polariscope objects with a new parabolic reflector, and anatomical injections of Australian animals, as also of the human body; some stereoscopic micro-photographs of Australian zoophytes, and some microscopic photographs of the moon at various phases, and, finally, microscopic photographs of eminent microscopists. This was a very choice selection. Mr. Sidney Gibbons exhibited a Powell and Lealand A 1 binocular, with opaque object; a Ross's A 1, with polariscope; an Oberhaüser; some micro-photographs, one of which showed the sheep scab on an enlarged scale, and the camera with which they were taken; and other instruments and interesting objects. The excellence of Mr. Gibbons' collection of instruments excited much admiration. A specimen of the red fungus, which has been so destructive to rye-grass in the Ballarat and Smeaton districts of late, was brought by Mr. Wallis, Secretary of Agriculture. Baron von Mueller says that the fungus is one of the Clavaria, of the family of cryptogamic fungi. Mr. Wallis is of opinion that this fresh plague is attributable to the system of laying down pastures adopted by some persons. Instead of sowing such a mixture of grasses as is sown in other countries, they generally produce rye-grass and clover only. After a time the clover dies off, and the rye-grass alone is left. Grown year after year on the same soil, this grass exhausts the elements which it requires, and the fungus disease is probably an outcome of the consequent weakness of the grass. It has only been discovered lately in this colony.

Dr. Robert Robertson, Hon. Secretary of the Society, exhibited,

with the aid of a Smith and Beck's student microscope, some rotifers. He also showed, with Ladd and Field's instruments, some fern spores and sections of woods.

A number of microscopical drawings was exhibited by Dr. Sturt, who presented them to the Society.

Mr. Robert Scott, by means of a Smith and Beck's instrument, exhibited some water plants, in the cells of which, though they were of no greater circumference than a hole pricked in paper with a fine needle, water could be seen circulating, going up one side and down the other. He also brought a copy of Hooke's 'Micrographia,' published in 1665. Dr. Wigg displayed a Ross's large monocular, a Smith and Beck's educational, and a Baker's small binocular, and also specimens of foraminifera. A curious exhibit was the tongue of a gasteropod, or sea-snail, shown by Mr. Stone. But still more curious were some objects shown by Mr. Barnard. Amongst these were the gizzard of a weevil, and the tongue of a tarantula. He also exhibited some Victorian foraminifera and diatoms. As most of his objects were of Victorian production, they attracted special notice.

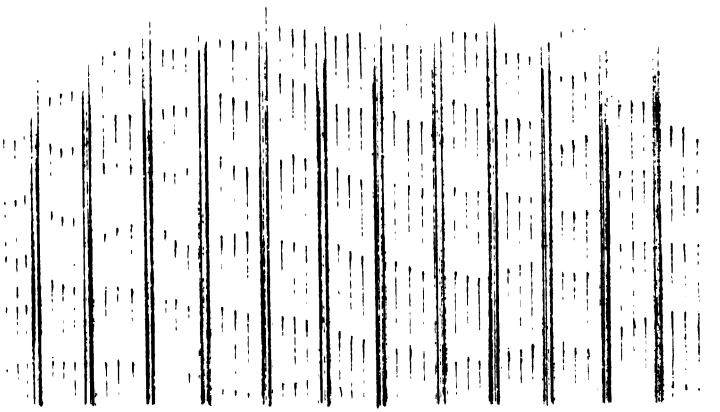
## Tower Hill Microscopical Club.

This Society was formed about a year ago in connection with the establishment of Messrs. Harrisons and Crosfield, of Great Tower Street, City, and is the first one in conjunction with the tea trade. On Tuesday the 27th Jan. it held a conversazione in the extensive sale room belonging to that firm. The objects exhibited numbered upwards of 150; and when it is mentioned that several members of the Royal, Quekett, South London, Morleys, and other Microscopical Societies, took part in the display, there is sufficient guarantee that the specimens were of the highest scientific importance, beyond being interesting to the brilliant assembly who met on the occasion. The following were amongst the objects, *viz.*:—

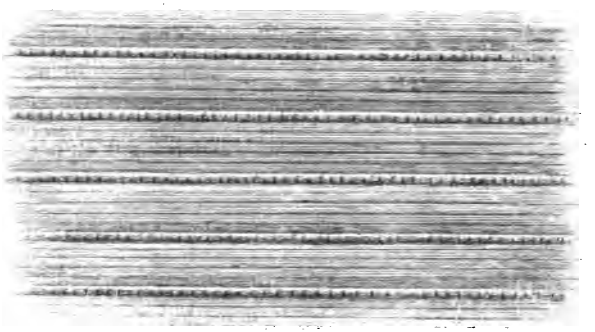
Section of blow-fly, showing tracheal and nervous system, dorsal vessel, and rectal papillæ. The drum of a frog's ear. *Puccinia malvacearum:* fungus on leaf of mallow first appeared in England 1873. Viscid thread of spider's web—Valisneria, showing the cyclosis. Egg of parasite of pheasant—*Lophopus cristallinus.* Crystals of salicine. Rolling stones polarized, &c., &c.

In addition to these microscopical objects there were several cases of very choice insects collected in various parts of Europe; sea-weeds carefully prepared, found in the Channel Islands; photographs of China, Italy, and other parts, of great value and interest, &c. The mechanical piping bulfinch, which was first shown at the Exhibition of 1862, also caused much amusement.

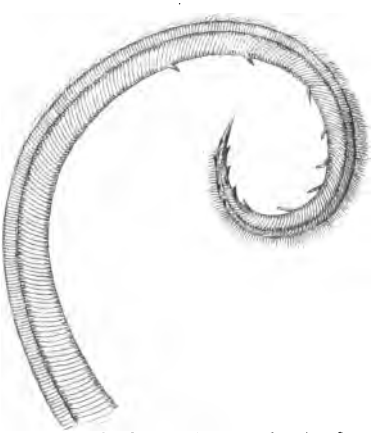
The beauty of the building was enhanced by the judicious floral display; and these attractions, with the addition of the band of the Great Tower Street Musicians, a Society emanating from the same firm, tended to make the evening enjoyable and instructive.



Scale of Lepisma.
Transmitted light. ⅛ objective.



Scale of Lepisma.
Seen with reflected light.



Left half of a curious proboscis of a Moth (name unknown) viewed from interior.

End of same viewed from the exterior surface

THE

# MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1874.

## I.—*The Scales of Lepisma as seen with Reflected and Transmitted Light.* By JOHN ANTHONY, M.D., F.R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *April* 1, 1874.)

PLATE LIX. (Upper portion).

POSSESSING a very fine slide of Lepisma scales, prepared for me some years ago by M. Charles Bourgogne, of Paris, but of which the covering glass was rather thick, I ventured to remove the said cover in order to substitute a thinner, and finding that to this thick covering glass adhered a considerable number of fine scales, I mounted it, reversed, upon an ordinary 3 × 1 inch glass slide, and took the opportunity of these facilities for making a series of careful observations of the upper and under surfaces of the Lepisma scale, both by reflected and by transmitted light.

I will venture to describe what I saw, inasmuch as there were appearances of structure on the surface of the scale and in the interspaces of the ribs which, so far as I know, have escaped previous examinations, and also present peculiar points of interest in analogy with other dermal tissues of insects; moreover, affording a comparatively difficult test of the defining powers of our higher modern objectives—those made some years ago certainly failing all but completely to show the appearances I am about to describe.

In the Lepisma scale, as usually mounted, I take it to be the under surface of the said scale which is nearest to the eye. I recognize the conclusions from Beck's experiments on the characters of the ribs, by which I think it is clearly made out that the quasi-parallel ribs must be on the upper surface of the scale only, while the radiating ribs arising from the quill portion of the scale are on the under surface only; and that there is no admixture or interweaving of these ribs towards the edge of the scale, but that the apparent "cross hatching" or "watered silk" appearance is produced by inequalities of surface alone—inequalities probably increased by the drying of the scales.

My attention was principally directed to the outer or upper surface of the Lepisma scales adhering to my reversed covering glass of the Bourgogne preparation; I examined it carefully, as if it were an opaque object, with direct light and with a variety of

powers, and I soon made out the appearances I have endeavoured to set down in Fig. 1, where the ribs seem irregularly beaded, but where is also seen over the whole surface of the scale a series of lines in the interspaces of the ribs, which would seem to correspond with longitudinal markings seen on an after-examination with transmitted light. A good $\frac{1}{8}$th objective showed these lines very easily, even sharper than I have ventured to draw them, and the $\frac{1}{8}$th and $\frac{1}{16}$th Powell and Lealand objectives enlarged, but did not otherwise modify these appearances.

Looked at by transmitted light, and after most careful attempts to avoid sources of error, the scale showed appearances such as I have endeavoured to represent in Fig. 2—appearances principally made out by the aid of a very fine modern $\frac{1}{8}$th and $\frac{1}{16}$th of Powell and Lealand, with the light made studiously as central as possible, and stops both of condenser and iris diaphragm, not too small, in order to avoid the risk of spectra. The markings to which I wish to call particular attention, it will be seen, bear some resemblance to the markings on some of the more delicate Podura scales, and I think appeared more distinctly on the surface of the uncovered scale: they seemed to me to be quasi-surface markings, and to be nearly on the same absolute plane as the parallel ribs—markings in or upon the surface of the membrane in the interspace between the one set of ribs, and not to be in any way in the substance of the scale.

The first appearance got in the examination of these markings is a mottled or patchy effect in the interspaces of the ribs, like ill-defined corrugations of certain scales of the Lepidoptera; and this effect was all that certain objectives, and particularly those of an ancient date, would show; but with my good modern $\frac{1}{8}$th and $\frac{1}{16}$th these blur-like appearances were resolved into markings seen quite as vividly as I have drawn them—markings which I could trace all over the scale, strictly parallel in every part to the quasi-parallel ribs—markings as on or contiguous to the surface, and visible even on that portion of the scale towards the edge where the "watered silk" appearance obtains, and where the obliquity of the radiating ribs is at a maximum; showing, I think, clearly that these markings have no connection whatever with the system of radiating ribs, but that whatever they may be, they belong to the surface of the scale alone. If we speculate on the nature of these appearances, of course they may be longitudinal plications of the membrane between the ribs, but they certainly bear a curious resemblance to some of the finer Podura markings. I do not think I manufactured the appearances I have endeavoured to describe by any effect of what is called "cooking" the illumination; for I carefully kept away from any obliquity of light, which I generally find more or less the source of most microscopic errors; and, as I have said, in addition to this precaution, I made use of quite a medium aperture both

in condenser and iris diaphragm, and I used also monochromatic light.

Now with regard to the reality of these appearances, I think the evidence is in their favour, inasmuch as with direct light a good $\frac{1}{10}$th or $\frac{1}{5}$th objective shows a distinct series of lines between, and parallel to, the ribs on the upper surface of the Lepisma scale, while very careful examination with transmitted light resolves these lines into Podura-like markings.

Experience has taught me to distrust appearances in the microscope seen by transmitted light alone; but of course where there is any corroboration from the use of direct light, it gives a proportionate value to examinations by high powers. These examinations are liable to all kinds of errors, and the results claimed cannot be too rigidly scrutinized. I give my observations for what they are worth; they are certainly curious, and may not be without use in calling attention to a not over easy object for testing the performances of modern high-power objectives.

I must just say a word about the parallel ribs being beaded more or less; they certainly show as such by reflected light, but not by transmitted light, except at the portions projecting beyond the end of the scale, where the rib certainly seems to taper off in beading.

## II.—*Note on a curious Proboscis of an unknown Moth.*

By S. J. McIntire, F.R.M.S.

(*Read before the* Royal Microscopical Society, *April* 1, 1874.)

Plate LIX. (Lower portion).

The notion entertained by most entomologists of the structure of the mouths of the Lepidoptera is that of a pair of long tubes locked intimately to each other by a multitude of minute hooks for the whole of their length. The space between the two halves, an artificial tube, is the passage by which the nectar of flowers is conveyed to the stomach, and the communication between this passage and the food to be eaten is effected by means of curious papillous appendages at the free extremities of the outer tubes, or, in the case of a vast number of the order, by apertures in the same situations. The haustellum of the cabbage butterfly is an example of the one type, and that of the swallow-tail butterfly of the other.

As regards the function of the organ, which in rest is spirally folded in front of the head between the palpi, it is generally accepted, I think, that it is fitted for suction, and has no power to penetrate any membrane, or other protective covering of a flower's nectary.

But I have met with an example which has puzzled me exceedingly, and, so far as I have been able to learn, is unique. Had I known this at the time, I should have taken pains to keep and identify the insect from which it was obtained. But being occupied in accumulating curious lepidopterous scales for the investigation of the *bead* question and its other intricacies, I neglected this duty, and am now obliged to tax my memory for particulars.

The insect was a drab-coloured moth, inclining to reddish brown, about 2½ inches across the wings, and I bought it among a quantity of damaged lepidoptera said to come from West Africa. The two halves of the haustellum had separated from each other for the greater portion of their length, and curled over to the right and left. The stoutness of the organ in comparison to its length caught my attention as curious, and so I mounted both halves in balsam. One I gave away, and the other I intend to present to the cabinet of this Society.

The points of difference between this object and the general structure of the haustella of the Lepidoptera are: first, the abundant fringes of hairs (some short, others long) on the outer margin of the spiral, near its extremity; and second, the structure of the termination of the tube. This ends in a hard chitinous point, and above it, externally, are several formidable recurved spines. When the two halves were locked together in life, the whole organ must have terminated in a hard and tolerably sharp point, strong enough to

pierce most vegetable structures. Penetration having been effected, the recurved spines would act as holdfasts and enable the insect to leisurely obtain its food. I cannot help thinking, moreover, that the unwary captor of such a moth might find to his surprise that it could in self-defence inflict a puncture of his skin by no means insignificant.

Being advised that the slide is worth bringing into notice, and likely to elicit some information, I have taken the liberty to bring it before this Society, and beg your acceptance of it, in order that those interested in the subject may be able to refer to it.

## III.—*An Instrument for excluding Extraneous Rays, in measuring Apertures of Microscope Object-glasses.*

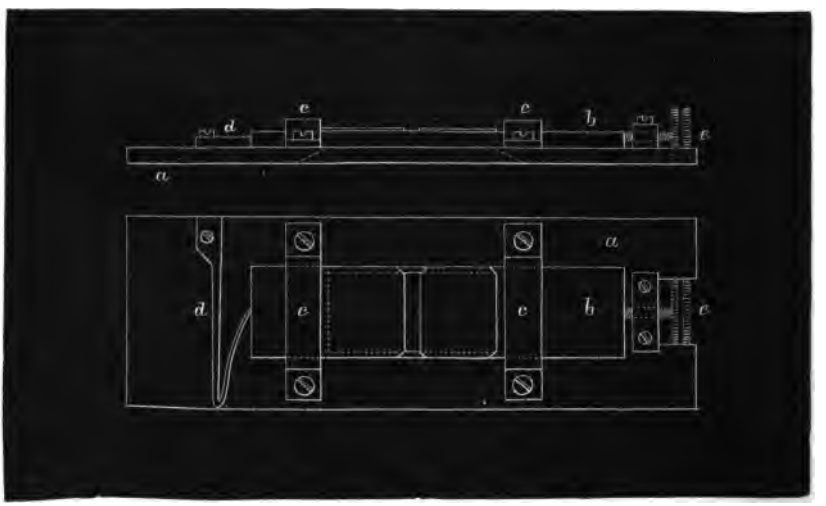
By F. H. Wenham, Vice-President R.M.S.

(*Read before the* Royal Microscopical Society, *April* 1, 1874.)

The angle of aperture of an object-glass is taken from the focal point through which the rays must pass for all degrees. Those entering at other angles within the plane of focus will give a false indication, from diffused light forming no image.

If a conical nozzle, having a small aperture in its apex, is placed over the front of an object-glass, the height of the cone being equal to the focal length, all such false light will be excluded, and image-forming rays only will enter at angles of any extent.

As there would be a difficulty in adapting such cones to every object-glass to be measured, an instrument has been contrived to meet the requirements. The traverse is horizontal during the measurement; therefore a vertical slit will serve instead of a circular stop. For high powers it is requisite that the metal edges of this slit should be exceedingly thin, and consequently must be secured from damage from contact with the object-glass or otherwise. The outer sides are required to be exactly in the focal plane during the measurement.




The cuts, full size in plan and side views, illustrate the arrangement. *a a* is a plate of brass with a central square opening chamfered away beneath so as to clear 170°; *b* is a slip of glass

sliding under two staples *c c*. At *d* is a spring for forcing the glass against the set-screw *e*. On the top surface of the glass there is a strip of platinum foil, ·001 thick, cemented on with Canada balsam. Fixed underneath the opposing staple there is a similar piece of foil. By turning up the set-screw to a stop the straight edges of the foils are brought in contact, and opened by reversing the screw.

This instrument is placed on the stage of a microscope, with the body horizontal and set on a wooden turn-table about ten inches in diameter, having its edge divided into degrees. The object-glass to be measured is focussed on the glass surface, and the slit adjusted so that its two edges just appear in the field of view, and by the rotation of the turn-table the degrees of aperture are read off. If the microscope has a thick stage this may cut off the rays for large angles. In this case a cell like an inverted live-box may be made to pass through to the under-side, to which the adjustable slit is fixed, and if the object-glass will not reach it must be extended by an adapter.

## IV.—*On the Construction of the Dark or Double-bordered Nerve Fibre.* By Dr. H. D. Schmidt, of New Orleans, U.S.A.

Plates LX., LXI., and LXII.

During a period of four years, beginning in the autumn of 1868, the greater part of my attention was devoted to microscopical researches into the structure of the nervous tissues. Of these researches, the subject of the present treatise, the double-bordered nerve fibre, forms a part.

The results obtained from these investigations, however, do not

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EXPLANATION OF PLATES LX., LXI., AND LXII.

Fig. 1, Pl. LX.—Peripheral nerve fibres of the frog, examined in serum; *a* and *b*, of the living, *c* of the recently killed, showing indentations of the double contour. Magnf. 500 diam.

Fig. 2, Pl. LX.—Peripheral nerve fibre of the mouse, prepared in serum immediately after death and subsequently treated with water. This fibre shows the fine dark line within the double contour. Magnf. 720 diam.

Fig. 3, Pl. LX.—Fresh peripheral nerve fibre of man, examined in water, showing the sack-like bulgings and corresponding windings of the delicate fibrils of the fibrillous layer. Magnf. 720 diam.

Fig. 4, Pl. LX.—Fresh peripheral nerve fibre of man, examined in water. On its open end, a portion of the fibrillous layer is seen to escape in loop-shaped bundles, showing distinctly their fine fibrillous character. On the side of the nerve fibre, the fibrillous mass is seen to escape through a minute rent in the tubular membrane in the form of a hernia. A considerable portion of the fibrillous layer has already escaped from the nerve fibre, in consequence of which the wavy fibrils crossing each other here and there appear very distinct. Magnf. 720 diam.

Fig. 5, Pl. LX.—Peripheral nerve fibre of the mouse with dilatations, examined in water immediately after death. Magnf. 720 diam.

Fig. 6, Pl. LX.—Fresh peripheral nerve fibre of the mouse, examined in water. In the interior of the fibre, the fibrillous bundles forming loops and covered by the semi-liquid medullary layer, characterized by its fatty lustre, as well as a number of single fibrils, all moving toward the open end of the nerve fibre, are seen. On the side we observe the fibrillous layer, a considerable portion of which has already been removed by the endosmotic current of the water at the lower end of the fibre. Magnf. 720 diam.

Fig. 7, Pl. LXI.—*a* and *b*, loop-like fibrillous bundles covered by the medullary layer, which escaped from the same nerve fibre. Magnf. 720 diam.

Fig. 8, Pl. LXI.—Fresh peripheral nerve fibre of the mouse, prepared in glycerine and subsequently treated with water. A portion of the fibrillous layer is here seen to escape from the open end of the fibre, in the form of fine fibrillous bundles, even single fibrils, all forming loops. The inner space of the latter is only filled by a very thin portion of the medullary layer, in consequence of which the individual fibrils appear very distinctly in this specimen. Magnf. 720 diam.

Fig. 9, Pl. LXI.—Peripheral nerve fibre of the same animal in glycerine. Magnf. 720 diam.

Fig. 10, Pl. LX.—The same nerve fibre, swollen by a subsequent treatment with water.

Fig. 11, Pl. LXI.—Nerve fibre of the same specimen with a hernia.

Fig. 12, Pl. LXI.—Fibrillous loops and coils; *a*, loops escaped from the preceding fibre (Fig. 11), and covered by a thin portion of the medullary layer; *b* and *c*, fibrillous coils, on which the medullary covering was partially dissolved by the action of water.

Fig. 13, Pl. LX.—Peripheral nerve fibre of the alligator, taken from a nerve having remained twenty-four hours in a solution of chromic acid; *a*, the entire

1.
a
b
c
13.
a
b
c
2.
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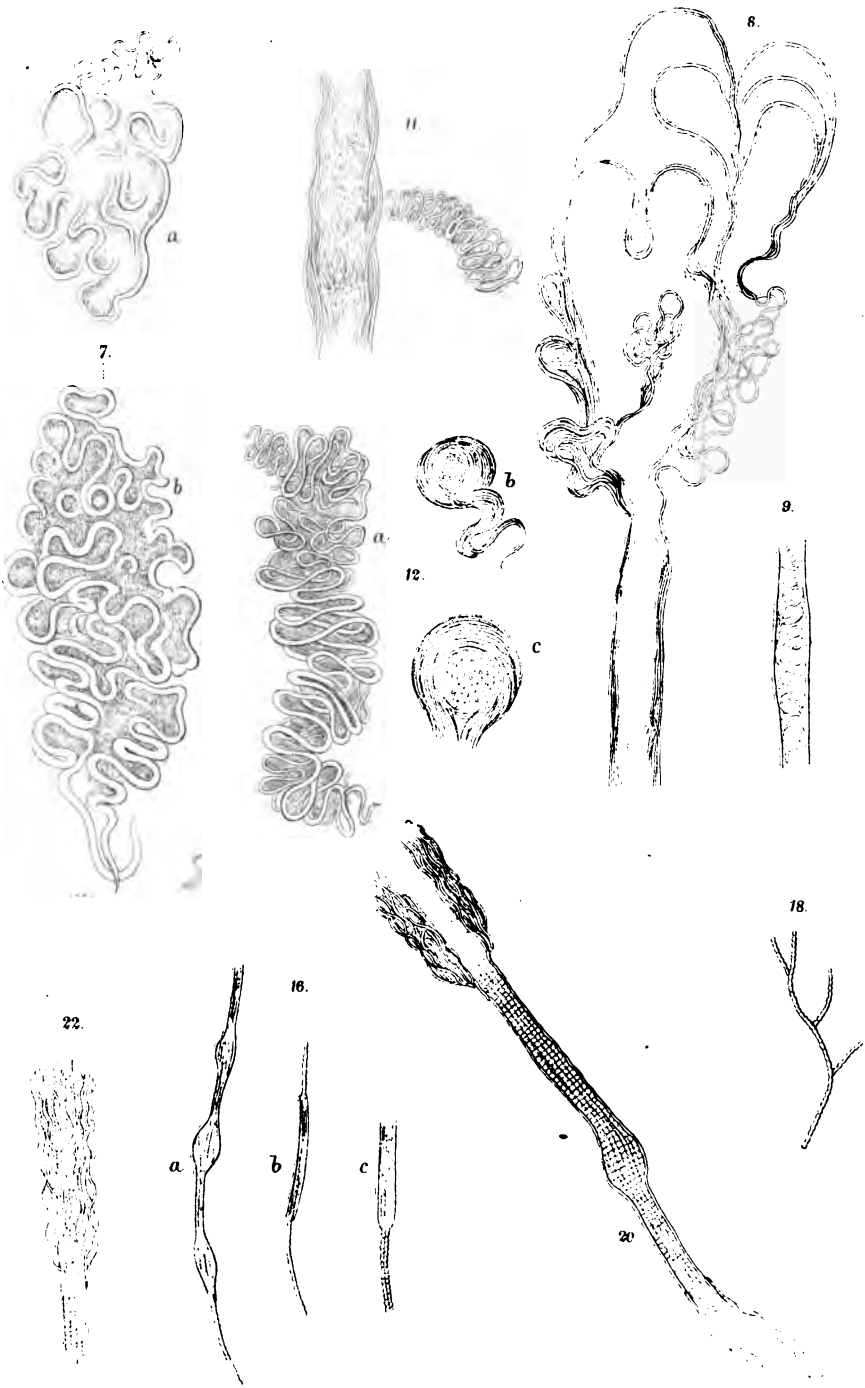
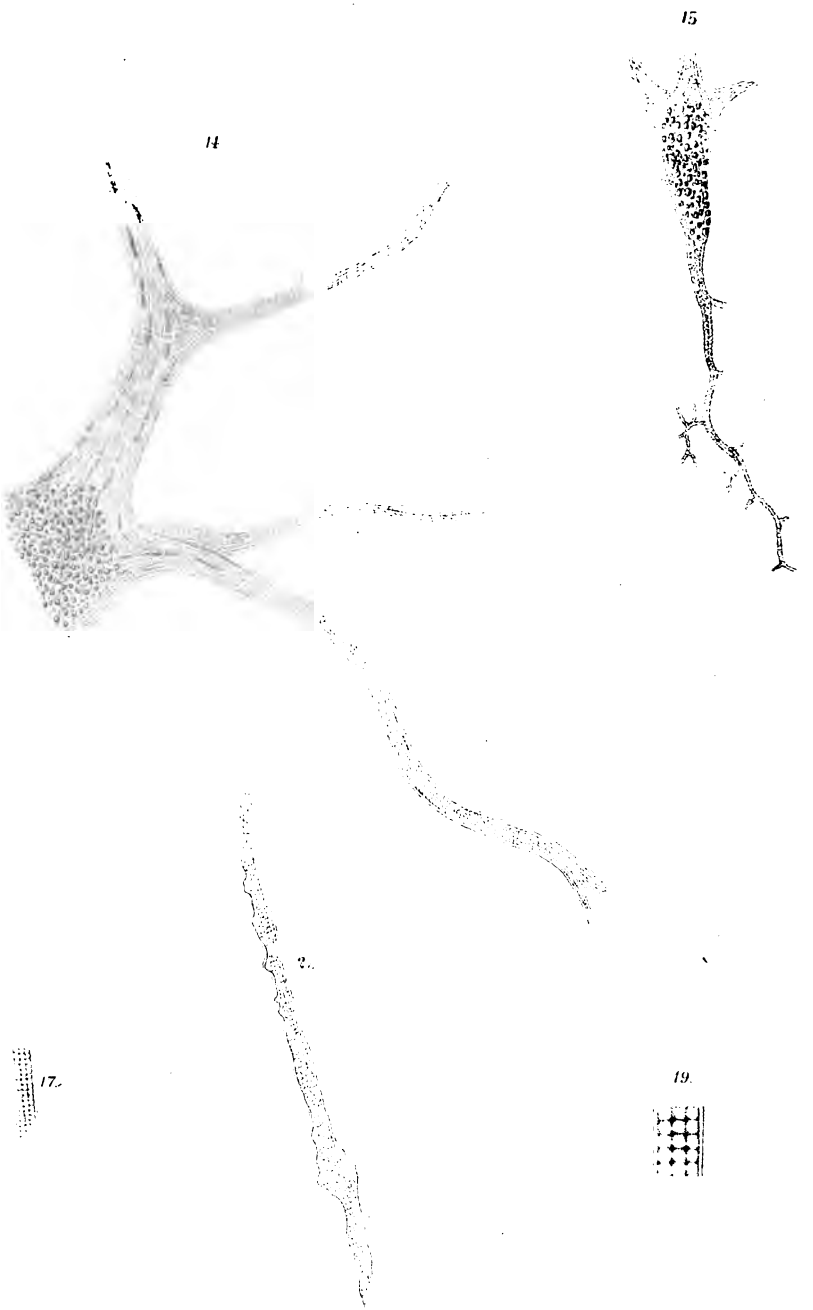
10
6.
3.
4




The structure of double bordered nerve fibr




Structure of double bordered nerve fibre.

W. West & Co° lith.

accord altogether with the existing views of other investigators; but, on the contrary, differ from them in many respects. It may be supposed, therefore, that my statements will be subjected to sharp criticisms from many sides; but knowing that the researches and observations forming the subject of this treatise were not made in a careless or superficial manner, I feel no hesitancy in placing them before the medical public in general, and my collaborators in particular.

All points which are at present considered as established facts I have avoided as much as possible, except in cases where the demonstration demanded a combination of the old with the new.

The more or less favourable results of all scientific investigations depend, as is known, to a certain extent upon the advantageous application of the means employed. I will consequently devote a short space to a few premonitory remarks, relating to the mode of investigation followed in my labours.

In microscopical examinations, the success does not alone depend upon a careful and correct preparation of the tissues to be examined, but it is quite as important to illuminate them in such a manner as to enable the eye to recognize the details of their construction.

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fibre, on which all its parts are distinctly recognized; *b* and *c*, fragments of axis cylinders, drawn out and partially torn by the needles during the manipulation. The granular-fibrillous structure is distinctly seen on these specimens. Magnf. 720 diam.

FIG. 14, PL. LXII.—Processes of a ganglionic body of the spinal marrow of man, having remained twenty-four hours in a weak chromic acid solution; on two of the ramifications some fibrils are seen to pass directly from one branch to the other. Magnf. 465 diam.

FIG. 15, PL. LXII.—Ganglionic body of the cortical layer of the cerebrum of man, taken from the margin of a thin section previously hardened in a solution of chromic acid. On this specimen the granular-fibrillous structure is also distinctly seen; the terminal ramifications of the long-pointed process belong to the terminal nervous network of the cortical layer. Magnf. 720 diam.

FIG. 16, PL. LXI.—Nerve fibres of the spinal marrow of man, with denuded axis cylinders; *a*, varicose fibre, the axis cylinder of which consists only of one granular fibril; *b*, straight fibre of the same size; *c*, fibre, the axis cylinder of which shows only two fibrils. Magnf. 720 diam.

FIG. 17, PL. LXII.—Fragment of the ramifications of a ganglionic body of the spinal marrow of man (fresh specimen treated with a very weak solution of chromic acid). Magnf. 970 diam.

FIG. 18, PL. LXI.—Fine terminal ramifications of a nerve fibre, coming from one of the lateral processes of a ganglionic body of the cortical layer of the cerebrum of man. Magnf. 720 diam.

FIG. 19, PL. LXII.—Diagram, representing an axis cylinder on a large scale.

FIG. 20, PL. LXI.—Axis cylinder of the spinal marrow of man, denuded of its coverings; on one point it has been pressed flat by the point of the needle. Magnf. 720 diam.

FIG. 21, PL. LXII.—Denuded axis cylinder of the spinal marrow of man, with folded sheath. Magnf. 720 diam.

FIG. 22, PL. LXI.—Nerve fibre from the anterior horns of the grey substance of the spinal marrow of man, showing the origin of the fibrils of the fibrillous layer from the sheath of the axis cylinder. Magnf. 720 diam.

The various parts of the nerve fibres and ganglionic bodies in the above figures are represented as more or less illuminated with oblique light.

Those microscopists engaged in the special study of the Diatomaceæ, and in the demonstration of those fine lines, elevations, and depressions found on their surfaces, have, for some time past, known the importance of a correct illumination, and accordingly always directed their attention to the construction or improvement of suitable accessory instruments for this purpose. In the examination of animal tissues, however, no particular attention, as far as I could ascertain, seems to have thus far been directed to the illumination; at least, I do not remember to have ever seen it made a subject of special remarks in those works on histology which have come under my notice. And still, the difference between the appearance of an object illuminated by central light, and that of the same illuminated by oblique light, is very considerable.

Knowing then from experience, that the success in resolving those fine lines of many Diatomaceæ depended, to a certain degree, upon the correct illumination of the object with oblique light—provided the angle of aperture of the objective is sufficiently large to admit such a portion of the oblique rays as is required for the purpose—I have employed it more or less for a number of years in my histological studies, and have likewise gained many advantages by it. If we examine, for instance, a number of fine bundles of fibrils, which, containing numerous nuclei or even vessels, cross each other above and below irregularly, we shall find it difficult to recognize the mutual relationships of these elements with the assistance of a central illumination. As all are transparent, and lie one above the other, one part of them is seen through the other, and the whole often presents the appearance of a complicated mass of outlines. The object appears quite differently when illuminated with oblique light by means of an achromatic prism.* It now resembles a drawing in *bas-relief*, for by means of the shades produced by the oblique illumination, and relieving the high lights, the true form of the elements of which the tissue consists is rendered more distinct to the eye, which in consequence is better enabled to recognize their mutual relations. The binocular microscope would prove to be still more advantageous for the examination of animal tissues than the prism, that is, if its construction should ever attain such a degree of perfection as to make it adaptable to a combination with objectives of high amplification.

In regard to the preparation and preservation of the material used in these researches, I particularly made use of a weak solution of chromic acid, but also, besides other reagents, of the chloride of gold, as recommended by *Cohnheim*. The latter proved to be especially useful in the examination of the termination of the nerves on the blood-vessels; but only in so far as, by its decomposition, the fine nerve fibres are coloured more or less purple, or even brown, in

* The prism which I use is that of *Abraham*.

proportion to the quantity of the metallic deposit. For the study of the more minute structure of the ganglionic bodies and the dark-bordered nerve fibres, however, I found it very uncertain. Without depreciating the assistance of reagents in general, I nevertheless always prefer the examination of the *fresh unchanged* tissue. In most cases, the advantages gained by the reagent are counterbalanced by the changes which the tissue undergoes; this is especially the case in the examination of the delicate nervous elements, and likewise of the embryonic tissues. It cannot be well disputed that those solutions of metallic salts, or other combinations so much employed in latter years, especially the nitrate of silver and chloride of gold, have proved to be of great advantage to the histologist; notwithstanding, however, the aspects which are obtained from tissues so prepared cannot be unconditionally relied upon; for the metallic deposit, as I know from experience, does not always take place evenly throughout all parts of the tissue. The advantage gained from such preparations consists mainly in this: that their various tissues may be better distinguished from each other, and that in consequence the study of their relations is much facilitated. It is thus that the chloride of gold has proved useful to me in the examination of the termination of nerves. For the study of the more minute structure of the tissues themselves, I must, however, always prefer a solution of chromic acid, for the reason that its action is more equally distributed throughout the whole tissue, and that no deposit obscuring the views takes place. Of all preserving agents which I have hitherto used, a weak solution of chromic acid seems to be attended with the least disadvantages. By the loss of a small part of water, which the tissues, as I suppose, must suffer by the action of the chromic acid, they gain, to a certain degree, in consistency, in consequence of which their contours become more sharply defined. Whatever I could possibly discover on specimens prepared with the chloride of gold, I have thus far always been able to demonstrate also either on the fresh specimen, or on such as were prepared with a chromic acid solution; but I could not always demonstrate on chloride of gold preparations what I saw on the former. It therefore appears to me somewhat hazardous to draw definite conclusions from observations made on specimens prepared with metallic salts, without having them confirmed by others, on such specimens as those just mentioned. The best results I have always obtained by minute dissections under the loupe, or by the making of fine transparent sections.

As I look upon the tissues of man, in general, as the type of the highest development, I therefore took the greater portion of the material used in these researches, in a condition as fresh as possible, from the human body, rarely longer than three or four hours after death, sometimes sooner. But besides this, I also used

the nervous tissues of a considerable number of animals, such as the ox, calf, guinea-pig, rabbit, mouse, frog, toad, alligator, turtle, lizard, snakes, and insects.

In examining in serum some *dark* or *double-bordered* nerve fibres of a freshly killed, or, as I have often done, of a still living animal,* under the microscope, we find them, as is well known, bordered by dark, sharply-defined double contours. The space between these contours, representing the greater portion of the entire nerve fibre, appears to a certain degree opaque, except where it borders on them; there it is seen as a clear stripe, becoming gradually fainter in the direction of the axis of the fibre (Fig. 1, *a* and *b*). In adding now, at the margin of the covering glass, a drop of water to the serum, a fine dark line is seen to appear in the interspace of the double contour itself (Fig. 2), dividing this, so to say, in two halves. The outer half, thus formed, is distinguished by a reddish, and the inner by a greenish hue, the difference in colour pointing to a different chemical composition. In pursuing the progressive changes which the nerve fibre undergoes by the action of the water, it will be found that the inner dark line, forming a part of the *original* double contour, is gradually dissolved and lost sight of; with it, of course, the inner, greenish shining half of the original double contour also disappears, while the outer half, with the loss of its reddish hue, remains. In consequence of these changes the fine dark median line, which at first appeared after the addition of water, dividing the original double contour, now forms the inner contour of the nerve fibre. At the same time, however, an essential change takes place in the main part of the nerve fibre, situated between the two, now very fine double contours. This consists in the appearance of certain irregular figures often described, which, examined with a sufficient amplification and central illumination, resemble somewhat an irregular network of fine tubular elements, as *Stilling* once described them; but, by a closer examination with an oblique illumination, they will be found to represent in reality a great number of fine fibrils, which in their usual wavy or tortuous course frequently cross each other, either singly or in the form of bundles, and thus give rise to the resemblance to a network (Figs. 3 to 6, and 9 to 11). We will now examine whether these fibrils owe their origin to a

* In the absence of an apparatus especially constructed for this purpose, a flat piece of cork, provided with a small round orifice covered by a suitable plate of thin glass, may be taken, and a frog fastened upon it in such a manner, that the bent knee will come to lie around the orifice. By a rapid dissection, a portion of the ischiatic and also the popliteal nerve are then exposed. Taking now some of the lymph from under the skin of the same or from another frog, it is put upon the previously slightly warmed glass plate and the coagulated fibrin removed from it. After this, the nerve is gently pulled over the glass plate into the remaining serum, its fibres quickly separated with finely pointed needles, and then covered by a small, also slightly warmed covering glass.

coagulation of the medullary substance of the nerve fibre, or whether they be real pre-existing elements.

In separating fresh nerve fibres by means of finely pointed needles from each other, a portion of the soft, semi-fluid medullary substance or *nerve-medulla* is almost always seen to escape from their torn and open ends. By a superficial examination with low magnifying powers, this appears as a homogeneous, semi-fluid substance, and as such it has been hitherto considered by almost all investigators of the nervous tissues, with the exception of *Stilling*. By a more careful examination, however, it will be discovered that the mass escaping is mostly composed of exceedingly fine and smooth fibrils, which, surrounded by a semi-fluid, finely granular substance, issue from the open end of the nerve fibre in the form of larger or smaller loops (Fig. 4). If it happens that the escaping mass be torn by the manipulation into larger or smaller portions, the fibrils will appear in the form of coils, frequently spiral in shape.

But it is not always that these masses of nerve-medulla, issuing from the nerve fibres, show at once their fibrillous composition; on the contrary, they often appear *apparently* bordered by a double contour of a fat-like lustre, and then resemble those so-called *myelin figures*, the origin of which was ascribed (*Liebreich*) to a decomposition of the protagon contained within the nerve fibre;* while again, their formation from the constituents of the brain in the fresh and undecomposed condition was decidedly denied † (*Koehler*) (Fig. 7, *a* and *b*). I cannot believe in the identity of these artificially produced myelin figures with those masses of medullary substance, escaping from the open ends of fresh nerve fibres, although I never had an opportunity of examining the former. In treating the escaping nerve-medulla with ether, acetic acid or other reagents, the characters peculiar to myelin figures will be lost, and they will appear in the above-described form of fibrillous coils. In many instances these fat-like, apparently double-contoured masses of different sizes are already seen in the interior of the nerve fibre, either stationary or floating, together with some fibrillous loops or coils, toward the open end of the fibre, from which they escape (Fig. 6).

In many cases, the fine wavy fibrils, appearing after the addition of water upon the surface of the nerve-medulla, and irregularly crossing each other, are not seen to extend throughout the whole nerve fibre; but, in addition, a number of narrow wavy bands or stripes of a greenish lustre are seen. These are particularly noticed at the sides of the fibre, where it is in focus, and are easily resolved into fibrillous bundles by the addition of more water, or, if this be not sufficient, certainly by acetic acid, alcohol, or ether.

* *Kühne*, 'Lehrbuch der Physiologischen Chemie,' 1866, p. 345.

† *Virchow* and *Hirsch*, 'Jahresbericht für das Jahr,' 1867, vol. i., p. 147.

Independent of the changes produced on the nerve fibre by the points of the needles during the manipulation, others, which cannot be ascribed to this cause, are observed to occur on its external surface; these consist of certain irregular, more or less sack-like bulgings (Figs. 3 and 5), appearing in the course of the fibre. They are particularly seen on fresh nerve fibres of man, of the ox, and of other higher vertebrata when prepared in water, and they always correspond, as is found by careful examination, to a loop or wave formed by the fine fibrils of the nerve-medulla. On nerve fibres of freshly-killed animals, on which the double contour has remained unchanged, certain changes, frequently described, are observed to occur, even when examined in serum. They consist in the double contour breaking off in certain places, and terminating in a sharp point in the direction of the axis of the fibre, while another arises in the form of a point a short distance above the end of the former (Fig. 1, *c*). These indentations, which are often observed on both double contours, and in an almost regular distance from and opposite to each other, represent folds of the tubular membrane and the nerve-medulla, probably produced by certain processes, to be directly explained. In the same manner, we frequently observe on the surface of the nerve fibre, grotesque, apparently double-bordered figures in the form of loops, hooks, &c., &c., which have also frequently been described.

All these changes, which the nerve-medulla often undergoes in regular succession, have been hitherto described by the older, and even some of the more recent investigators of the nervous tissues as spherical, granulous, clodded masses, produced by coagulation; and as such they are still spoken of in recent works on histology. That the true nature of these clods has not been recognized before this, can hardly be explained otherwise than that they have been examined with amplifications too low—250 to 300 diameters—and with insufficient illumination.

As far as I am now able to judge of the *anatomical* composition of the nerve-medulla by my numerous examinations relating to the subject, it seems to consist of two layers, distinctly differing from each other. The *outer* one of these shows a structure composed of very delicate and smooth fibrils, about $\frac{1}{1200}$ mm. in thickness, and arranged parallel and very close to each other; they can be demonstrated without difficulty and almost under all circumstances. The *inner* one surrounds directly the axis cylinder, and consists of a finely granular, amorphous, and semi-liquid substance. We will designate the former as the *fibrillous*, and the latter as the *medullary* layer.

Although I doubt but little the pre-existence of these two layers of the nerve-medulla, I do not venture to defend this view as the only true one, but certain it is, that they manifest themselves

shortly after death on the addition of water. According to my observations, the fibrillous layer consists of those fibrils just described, which are intimately connected, not only to each other, but also to the inner surface of the tubular membrane, by an intermediate substance, soluble in water. The mutual connection of these two membranes forms the original double contour of the nerve fibre. The appearance of the double contour has been ascribed, like that of those clods, hooks, or other figures, to a coagulation of the outermost portion of the nerve-medulla, caused by an access of the atmosphere,—and it is not very long ago that even the formation of the axis cylinder itself was ascribed to this cause, and the entire double-contoured nerve fibre regarded as consisting only of a homogeneous semi-fluid substance, enclosed within the tubular membrane. Again, it is said that the nerve fibre, immediately after its removal from the living organism, and without the addition of reagents causing visible changes, has been seen in the form of a transparent cylinder of a dull lustre, bordered by simple contours and devoid of any other distinguishing character.* In the transparent eyelid of the frog and in the tail of the tadpole, the nerve fibres, it is said, have been seen—but only rarely—in the form of homogeneous, clear, milk glass-like threads.† The observation on the tail of the tadpole can hardly be relied upon, as it is not very probable that, in this instance, the double-bordered nerve fibre had obtained its full development, but may rather still have existed in the form of a naked axis cylinder.

Whether the statements last mentioned, together with others, are well founded, or whether they simply rest upon some optical delusions, I venture not to decide; but as far as my own researches are concerned, I must say that I have never seen the nerve fibre in question in the form of a clear thread with simple contours,—not even in the uninjured bundles of nerve fibres of the ischiatic nerve of the living frog. Nevertheless, it might be possible that the fibres of an exposed nerve undergo a sudden change by the momentary contact of the atmosphere. But if this were the case, it must also be presumed that the electro-motory behaviour of these fibres would differ from that of those belonging to a nerve still occupying its place in the uninjured animal body,—in consequence of which many observations relating to the electricity of the nerves would appear unfounded. In the same manner, the axis cylinder also should possess a different degree of conducting power in a fluid condition, than in a state of coagulation.

Considering, therefore, the difficulty of deciding this question, and, further, the different views still held by a number of chemists regarding the chemical composition of the nerve-medulla in general,

* *Funke*, 'Lehrbuch der Physiologie,' 4th edit., vol. i., p. 661.

† *Frey*, 'Handbuch der Histologie u. Histochemie,' 2nd edit., p. 353.

it seems to me more practical to be guided only by the results of ocular demonstration, which alone for the present can be relied upon. For this reason, we shall try to give from an anatomical point of view, a reasonable explanation of those successive changes occurring within the double-contoured nerve fibre corresponding with our own observations. I presume, therefore, that the fibrils of the fibrillous layer are formed of a consistent, though very soft albuminous body, while the semi-fluid medullary layer consists of a substance which is especially distinguished by its fat-like lustre and by its partial solubility in water. As regards the organic constituents of these substances, it must be left to organic chemistry to determine, for the above view is only based upon the behaviour of the nerve-medulla to the action of various reagents, as seen under the microscope. Now in examining a number of nerve fibres in serum, no particular changes will be observed to take place within them, because no considerable difference probably exists in the density of this liquid and that of the semi-fluid medullary layer, and also the intermediate substance of the fibrillous layer, in consequence of which no endosmosis occurs. The changes, manifesting themselves sometimes soon after, by the appearance of those indentations or folds above mentioned, may be ascribed to an evaporation of the serum, occurring at the margin of the covering glass, increasing its density and giving rise to a slight exosmosis on those points of the nerve fibre.

The first change which is seen to occur on the nerve fibres, prepared in serum, after the addition of water, is the appearance of that fine line already mentioned (Fig. 2), dividing the double contour into two parts.* It is produced by the action of the water, which, obeying the law of endosmosis, penetrates through the outer part of the double contour, representing the tubular membrane, and, dissolving the connecting medium, causes this to be separated from the inner, the fibrillous layer of the nerve-medulla. With the continued advance of the water toward the axis of the nerve fibre, that portion of intermediate substance which connects the individual fibrils of the fibrillous layer to each other, is also dissolved, in consequence of which they are rendered, either singly or still holding together in the form of small bundles, visible to the eye of the observer. Penetrating still farther into the interior of the fibre, it now reaches the medullary layer, and dissolving the albuminous constituents of this body, finally causes those disturbances and destructions, especially in the fibrillous layer, which have already been partially described above. In order to explain these

* This fine dark line, which divides the outer part of the nerve fibre, represented by the double contour, into two layers differing in their refraction of light, and which *Stilling* already described more than sixteen years ago, seems, as far as I know, to have been overlooked or ignored by most histologists.

disturbances satisfactorily, it must be supposed that various conditions and circumstances may interfere with the endosmotic process, so that it cannot go on at the same rate at all points of the nerve fibre. At those places, therefore, where the current is strongest, the penetrating water will momentarily be collected, dilate the tubular membrane, and cause—especially in the vicinity of a place where the endosmosis is feeble—those sack-like bulgings (Fig. 3). The same process causes also the formation of those loops and coils, composed of the exceedingly delicate fibrils of the fibrillous layer, which are observed as well in the interior of the nerve fibre as when escaping from its open ends (Fig. 4). Sometimes the tubular membrane is slightly torn, offering an opportunity to observe distinctly the issue of the fibrillous loops in the form of a hernia from the opening (Fig. 4). To explain the formation of these loops and coils more perfectly, it must be taken into consideration that the dissolution of the intermediate substance which binds the fibrils to each other, does not everywhere take place in the same degree, and that accordingly, the entire fibrillous layer is not separated into single fibrils at once; on the contrary, many of the latter are still observed to adhere together in the form of fine bundles. In an equally irregular manner, the separation of the fibrils from the inner surface of the tubular membrane may take place, in which case those portions already loosened would be taken along in the form of loops by the endosmotic current. Accordingly, long loops are often seen in the interior of the nerve fibre, floating towards its open ends with the current (Fig. 6). The substance of the fibrils is so very delicate and soft, that they in consequence will undergo new changes in form (caused by the current), and in this manner the most irregular coils, loops, and windings are frequently formed at every obstacle which they meet in their course,—yes, in places where they cross each other, they will run into each other (Figs. 10 and 11), and become even *apparently fused* into an irregular clod.

While a portion of the fibrillous layer is carried in the form of loops and coils to the open end of the fibre by the current of the water penetrating through the tubular membrane, the rest remains in its original place, where it is now seen as fine, more or less wave-like bundles of fibrils (Figs. 5, 6, 10, and 11). The disturbances which this layer suffers through the endosmosis of the water, are observed in a greater or lesser degree in different animals; they probably stand in some relationship with the delicacy of its composition. In man and other higher vertebrata in general, the fibrils appear to be more delicate than in the amphibia. In the alligator and turtle especially, they seem nearly to maintain their original position (Fig. 13).

Those apparently double-bordered masses of a fat-like, greenish

lustre, which are seen to escape from the interior through the open ends of the nerve fibre (Fig. 6), consist of nothing else but those fibrillous coils, loops, &c., &c., just described, *covered by the semi-fluid medullary layer which possesses a fat-like lustre.* The cause of the *apparent* double contour, by which they are bordered, has also, like that of the entire nerve fibre, been ascribed to coagulation. It will be found, however, that while that of the nerve fibre is distinguished by two dark, sharply-defined lines with a clear space between them, that of those irregularly-shaped fat-like masses or supposed myelin figures possesses a *greenish lustre*, and is mostly represented by only *one* real border line. A critical examination will show that the contour always corresponds to *a winding of a fibrillous bundle* (Figs. 6, 7, 11, and 12), and that in the most cases the *individual fibrils* may be recognized (Fig. 8). The clear space between the shining greenish margins and windings is of a bluish-grey hue, belonging to the peculiar lustre of fatty masses, as they appear under the microscope, and represents the covering of the adhering portion of medullary layer. To remove all doubts concerning the nature of these bodies, some drops of ether are made to run under the covering glass, while a small piece of blotting-paper is applied opposite to this point; the ether at once dissolves the fat-like covering and causes the individual fibrils to appear. By a continued action of this reagent, however, the latter will coagulate and appear granulous.

When nerve fibres are prepared and examined in serum, and only gradually exposed to the action of water, wavy bands of a greenish lustre are frequently observed to remain behind in their interior. These also represent fibrillous bundles covered by the shining, fat-like constituents of the medullary layer; their fibrillous character is proved by the application of ether, or, even, a continued action of water upon them will bring out the individual fibrils of which they are composed. Judging from the last-mentioned fact, the substance of the medullary layer must be partially soluble in water.

Besides these constituents of the medullary layer which are distinguished by their fatty lustre, a great number of fine pale molecules or granules are observed attached to the fibrillous loops escaping from the open end of the nerve fibre (Figs. 4 and 13). These probably consist of some consistent albuminous body, but whether in a natural or coagulated state, I do not venture to decide. In the nerve fibres of the alligator or turtle, they seem to be most distinct.

The first traces of decomposition of the nerve-medulla in the interior of the nerve fibre manifest themselves by the appearance of a number of vesicles (Fig. 4). They show a fine double contour, and vary considerably in their diameter, measuring from $\frac{1}{600}$ to

about $\frac{1}{150}$ mm. In the interior of the fibre they appear in a hexagonal form, but after they have made their escape through its open ends, they resume their original round form. These apparent vesicles or cells probably consist of certain fat-like constituents of the medullary layer, and are formed during the separation of the different bodies of which it is composed. They are especially met with in the nerve fibres of man, of the ox, or of such animals, where opportunity of examining them immediately after death is wanting, for which reason they may also be regarded as a product of a natural decomposition. Treated with ether, they are seen to dissolve within the fibre, or, assuming a round form, to travel toward its open end. When fresh nerve fibres, after having remained for some hours in ether, are examined, their fat-like constituents will be found to have almost entirely disappeared, and, besides a number of fibrils, the remains of the albuminous constituents are only met with in the form of coagulated molecules or small irregularly-shaped bodies in their interior.

A weak solution of chromic acid also seems to facilitate the decomposition of the medullary layer, while it renders the fibrils of the fibrillous layer more consistent. If, therefore, some bundles of fresh nerve fibres are put in such a solution and examined on the following day, it will be found that the fibrils of the fibrillous layer have only slightly assumed a wavy appearance, and almost entirely preserved their original parallel position. In the axis of the nerve fibre, the naked axis cylinder is seen surrounded by a finely-granular transparent liquid, and frequently found projecting for some distance beyond the open end of the fibre (Fig. 13). In some cases, a number of the above-described hexagonal vesicles are also observed, on which, however, no further changes in form can now be discovered by the application of ether. From this, it seems that they are formed during the decomposition of the medullary layer, and mainly consist of the fat-like constituents with an albuminous covering, which, in these instances, is coagulated by the action of the chromic acid. On nerve fibres of the ox, which had been laying for ten months in a mixture of nine parts of water and one of alcohol, the same observations were made. While the tubular membrane was preserved, the contents of the fibres, with the exception of some accumulations of these hexagonal vesicles, had entirely disappeared. Although, by the addition of ether, larger and smaller fat-like masses were seen to escape from the open ends, no change of form was observed in the vesicles, they were only rendered a little clearer.

From what has been said thus far, it is seen that in examining fresh nerve fibres in water an endosmotic current of this liquid takes place, in consequence of which they swell and frequently gain considerably in diameter. Just the contrary is observed on fresh

nerve fibres, when prepared and examined in glycerine. As the density of this liquid probably exceeds that of the liquid part of the nerve-medulla, a current must take place in an opposite direction, in consequence of which their diameter is considerably reduced, and they furthermore assume a fat-like, opaque and greenish appearance (Fig. 9). The double contour also loses in diameter. By a subsequent addition of water and the removal of the glycerine by means of a small piece of blotting-paper, the endosmotic current goes again from the exterior to the interior, the nerve fibre loses its fat-like lustre by the entering water, swells even frequently beyond its normal diameter, and appears finally in every respect as if it had been treated with water from the beginning of the examination (Figs. 8 and 10).

The observations on the construction of the double-bordered nerve fibre, described in the preceding pages, related mainly to those of the periphery. The fibres of the central organs of the nervous system possess the same structure as the latter, but differ from them only in their smaller diameter, which, in the finest fibres of the grey substance of the brain and spinal marrow, amounts to about $\frac{1}{800}$, or sometimes to not more than $\frac{1}{900}$ mm., while in the larger ones of the spinal marrow it only exceptionally exceeds $\frac{1}{120}$ mm. Another peculiar trait of character of the central nerve fibres consists in those well-known varicosities which appear in their course; these, however, occur principally on the finer fibres, on which they are frequently observed at such a regular distance from each other, that one might be tempted to regard them as natural products. I have never succeeded in demonstrating a tubular membrane on the nerve fibres of the brain and spinal marrow, but notwithstanding, I would not venture to deny the existence of it entirely. Quite different is it with the fine fibrils of the fibrillous layer of the nerve-medulla, which, except on the finest fibres, can always be seen as distinctly as on the double-bordered nerve fibres of the periphery (Fig. 20). On the larger fibres, they are seen to escape from their open ends in the same manner as on the peripheral nerve fibres, that is, in the form of loops, coils, &c., &c. In the interior of the nerve fibre, however, these loops are not observed so often, because, as I imagine, the small diameter of the entire fibre does not allow their formation.

As regards now the formation of those peculiar varicosities or dilatations of these nerve fibres, it is difficult to find a satisfactory explanation for this phenomenon, especially as they do not appear as irregular sack-like bulgings, but—mostly in the finer fibres—in a more or less regular form. In the larger fibres these varicosities are observed more rarely, and when they do occur, it is more in an indefinite irregular form. The most reasonable explanation seems to be, that their formation is due to some decomposition taking

place at those varicose places, giving rise to the development of gases which finally cause the dilatation; or it may be attributed to an endosmosis of liquids. These hypotheses would naturally require the supposition of the *existence of a tubular membrane.* The probability of these varicosities being produced by decomposition, or some similar process, may be inferred from the fact that they are especially met with in specimens of the fresh substance of the brain or spinal marrow examined in water, while in others of the same substance, hardened in a solution of chromic acid, they are only rarely observed.

The fine fibrils of the fibrillous layer I have thus far not succeeded in demonstrating on the finer nerve fibres of the central organs, though a distinct double contour may be always recognized on them. The cause thereof may well be found in the very inconsiderable thickness or even absence of the medullary layer, in consequence of which the fibrillous layer would directly surround the axis cylinder. That this is really the case may be inferred from the fact that the inner line of the double contour also forms the border of the axis cylinder. The absence, or even the presence of the medullary layer in an inconsiderable quantity, would render the endosmotic process more difficult, and enable the fibrils to remain in their original parallel position; only on those varicose places the entering water would accumulate, and cause those oval dilatations of the fibrillous layer and tubular membrane. In many cases the axis cylinder is distinctly recognized in the axis of the dilatation (Fig. 16, *a*).

Although, as far as we have seen, no considerable difference seems to exist in the anatomical composition of the double-contoured nerve fibres of the central organs and that of the peripheral fibres, I believe I have observed that the former are able to resist the action of water or a weak chromic acid solution for a longer time than the latter. In the examination of nervous tissues of the brain and spinal marrow in water, or of such which had remained for a short time in a weak solution of chromic acid, a number of nerve fibres, the double contour of which is unchanged, is always met with; a circumstance not often occurring with the fibres of the peripheral nerves under the same conditions. The medullary layer of the former also seems to preserve its greyish lustre for a longer time than that of the latter, which may be attributed to a greater amount of fat-like constituents in their chemical composition, increasing their power of resistance to the solving action of the water.

The fibrils of the fibrillous layer of the central double-bordered nerve fibres differ in nothing from those of the peripheral fibres. In the anterior horns of the grey substance of the spinal marrow, they are seen to escape from the open ends of the peripheral nerve

fibres arising there, and to accumulate in considerable masses in the form of wavy loops. Their substance is so delicate as to allow them after a while to adhere to, or apparently fuse with each other, and thus, especially through the action of chromic or acetic acid, to assume the form of a fine network.

As regards now the nature of these fibrils of the fibrillous layer of the nerve-medulla in general, we can, in considering their general behaviour, hardly come to any other conclusion than that they probably are nervous elements. It could not be well supposed that their formation is due to a coagulation of the nerve-medulla, for they do not appear like fibrin, in the form of an irregular fibrillous, or like albumen, in that of a granulous coagulum, but are always observed to run parallel to each other, either in waves or loops. Acetic acid, which, as is known, causes fibrin as well as the white fibrous tissue to swell, and the walls of cells to disappear, leaves these fibrils unchanged; on the contrary, they appear in many instances after the treatment of this reagent, only more distinct.

We will now turn our attention to the most important part of the double-bordered nerve fibre, the so-called *axis cylinder*. Not many years ago, the pre-existence of this part of the nerve fibre was, as it is known, still disputed, and—probably for the want of a better explanation—it was regarded by some histologists as a product of coagulation. Additional, more thorough researches, however, not only disproved this view, but further demonstrated that it represented the true nerve fibre. Nevertheless, it was still looked upon as a homogeneous body; until, some years ago, this view also became untenable through *Max Schultze's* discovery of the fibrillous structure of the axis cylinder. According to the view of this investigator, the axis cylinder consists, as is now generally known, of a number of exceedingly fine and *smooth* fibrils, which are united into a bundle through an inter-fibrillous, *finely granular* substance. The conclusion to which I have come from my own examinations, regarding the structure of the axis cylinder, confirms on the whole, it is true, the view just mentioned, but deviates from it in other respects in some essential points. But, notwithstanding, it seems almost that this deviation may possibly be due to a difference in the mode of examination, as my examinations were, as already stated, principally made with an oblique illumination by means of the achromatic prism.

According to the results of these investigations, the axis cylinder consists of *minute granules* about $\frac{1}{1200}$ mm. in diameter, which are *arranged in regular rows* and united by a *homogeneous* interfibrillous substance, and thus form a bundle of *granular fibrils*. Each axis cylinder is, therefore, according to its thickness, composed of a number of these granular fibrils, which, united into a bundle, are enclosed within a *distinct, delicate membranous sheath*.

The difference between the view of *Max Schultze* and my own consists therefore only in the nature of the fibrils and the inter-fibrillous substance, and also in the absence and presence of a sheath. We will now pass over to the details of the examinations, and try, by means of sufficient proofs and reasons, to demonstrate the granular nature of these fibrils as a fact.

With the assistance of a *first-class* objective and a well-adapted illumination of the object, especially with oblique light, the granular-fibrillous composition of the axis cylinders, projecting from the torn ends of many fresh nerve fibres, may even without difficulty be recognized. But better specimens may be obtained from nerves or spinal marrow, that have been laying in a very weak solution of chromic acid from about twelve to twenty-four hours. This reagent seems to promote the decomposition of the medullary layer, while it produces an opposite effect upon the axis cylinder. In carefully dissecting, now, a bundle of such nerve fibres with very finely-pointed needles, it always happens, that during the manipulation a number of axis cylinders are drawn out to some extent from their respective fibres; but frequently also that one of them becomes torn or pressed by the point of the needle, offering an opportunity of examining somewhat more accurately the details of its construction. In this way I came to recognize, some years ago, on some axis cylinders, partially torn and drawn out from their respective peripheral nerve fibres of the alligator, their granular-fibrillous structure. In referring, therefore, to Fig. 13, some of these axis cylinders and also a nerve fibre will be found represented, which I copied from nature at that time; *a* represents the entire nerve fibre, showing all its parts; but as it was considerably magnified when drawn, it was necessary, by slightly altering the adjustment, to bring the axis cylinder into focus; the drawing represents, therefore, all parts of the fibre almost in the same focus. At *b*, a piece of axis cylinder is seen which, near its middle, has been pressed flat and slightly torn by the point of a needle, in consequence of which a number of granules were displaced and escaped through the orifice thus produced. The small fragment *c* shows even the rent and the granules displaced from their natural fibrillous arrangement. The membranous sheath manifests itself on each of the specimens by a fine double contour. As the specimen was removed from the body immediately after the death of the animal and put into a solution of chromic acid, the fine fibrils of the fibrillous layer remained to a certain extent in their natural position. They can be seen, of course, only on the sides of the fibre, but by a slight change of the adjustment they could be traced, though not as distinctly, over the whole nerve fibre.

Among the smaller as well as the larger nerve fibres of the spinal marrow, a number of axis cylinders, drawn out from their coverings

or denuded from them (Figs. 16, 17, and 20), are always met with, on which their granular-fibrillous structure can be distinctly recognized. In man especially, they can always be demonstrated; the same is the case with the sheath surrounding the bundle of fibrils, which in many instances manifests itself by a very sharply-defined double contour.

In examining, now, one of these axis cylinders, previously prepared with a weak chromic acid solution, with an amplification of about 275 to 300 diameters, and with central illumination, nothing more is recognized on it than an exceedingly fine, longitudinal striation; but in increasing the magnifying power to about 500 diameters, the aspect is changed, and we observe in the course of the striæ a number of dark points, separated from each other by minute interspaces, and which, with this illumination, may be easily looked upon as minute granules (Fig. 14). The view thus obtained corresponds also with *Max Schultze's* description of the structure of the axis cylinder. If now, however, the same object is illuminated with oblique light by means of an achromatic prism, it will be found that the dark points are in reality no granules, but are only produced by minute depressions. At the same time, fine, pale shadows will be observed, proceeding from the latter and crossing the fibrils of the axis cylinder, while, between these lines of shadow, minute elevations appear in the form of clear, shining points, giving to the whole the appearance of rows of minute granules partially fused with each other (Figs. 15, 16, *c*—17, and 20). In referring to the diagram (Fig. 19), which represents, on a larger scale, an axis cylinder obliquely illuminated, we shall observe its three fibrils, composed of granules, running in such a manner parallel to each other that, transversely, their granules come to lay also in a straight line. As the intermediate substance connects the latter principally at those points where they come in direct contact with each other, the greatest depression will naturally be found at such points where they are most distant from each other, that is, in the centre between every four granules, placed in immediate apposition to each other. And it is just at this place where the above-mentioned dark, granule-like points are seen. If we now imagine the whole illuminated with oblique light, it follows that one side of the granules will be more strongly illuminated than the other, by which their roundness is brought out; while, at the same time, those depressions between every four granules must appear darkest. The whole arrangement and appearance of these granules resembles somewhat the hexagonal elevations seen on some of the Diatomaceæ, as, for instance, on the *Pleurosigma angulatum*.

The granular-fibrillous structure of the axis cylinders is very distinctly recognized on those of the finer nerve fibres of the spinal marrow or the brain, consisting of two or even only one fibril, and

on which no sheath is any more to be seen (Fig. 16). The contours of these, namely, do not appear as straight lines, but as a row of small convexities, each of which is produced by a projecting granule. With oblique illumination, the whole row of granules may already be recognized with an amplification of 500 diameters. With one of 700 diameters, or even higher (Fig. 17), the granular structure of the fibrils is placed beyond doubt. On the finest ramifications of some processes of the ganglionic bodies of the cortical substance of the brain, or in the terminations of fine nerve fibres in its nervous network, the granular nature is unmistakable (Fig. 15).

The fibrils of the axis cylinder extend through the processes of a ganglionic body over its surface. A considerable number of them connect the processes with each other, *i. e.* a part of the fibrils of one process arriving at the ganglionic body, pass over its surface and take part in the formation of other processes. The course of the rest I must leave untouched for the present.

In the axis cylinders the fibrils are arranged closely to each other. But this is not the case with the thicker processes, in which they are placed more loosely alongside of each other (Fig. 14); accordingly, single fibrils are frequently observed on the torn ends of these processes, reaching a little distance beyond their neighbours, and affording one an opportunity of becoming convinced of their granular-fibrillous structure. Arriving at the roots of the processes, the fibrils diverge in their course, to pass, as already mentioned, in various directions over the surface of the ganglionic body. At this separation, however, the entire process does not become divided into single fibrils, but it seems rather that the latter separate from each other in pairs, in which form they are also observed on the surface of the ganglionic body, especially along its margin.* On torn ganglionic bodies, it may be further observed, that these pairs of fibrils may also become broken in a longitudinal direction into pairs of granules, so that groups composed of four granules may be formed. In examining, therefore, the surface of a ganglionic body illuminated obliquely, with an amplification of 700 diameters, it will appear to be composed of minute groups of granules. This is especially the case on specimens prepared with a solution of chromic acid; on fresh ganglionic bodies, however, these groups of granules appear almost as so many minute polygonal bodies with a dark point in the centre. As such I had also regarded them until a year ago, when by a careful re-examination of the subject I found that the ganglionic bodies, as well as the processes proceeding from them, were composed of the same fine granular fibrils as the smaller axis cylinders.

* This separation may perhaps also take place in the form of small bundles composed of three or four fibrils, which is, however, difficult to determine, as never more than two are presented to view.

As has been said before, the granular-fibrillous structure of the axis cylinder may be recognized on almost all fresh specimens, projecting from the nerve fibres; where this is not the case, the cause may probably be found in a delicate covering of the semi-liquid medullary layer of the nerve-medulla, which adhered to the axis cylinder when drawn out from the nerve fibre. If, therefore, a drop of chromic acid solution, sufficiently diluted to prevent a colouring of the specimen, is suffered to run under the covering glass, the granules will come out. The same occurs on the processes of the ganglionic bodies. The chromic acid solution, weak as it is, probably abstracts from the intermediate substance of the granules a small portion of its water, in consequence of which the latter will project more. On the ganglionic bodies of the spinal marrow, examined twenty-four hours or longer after death, we can, in consequence of the commencing decomposition, obtain further evidence of the granular nature of the fibrils. As the intermediate substance, namely, is the first to undergo decomposition, the granules may be seen almost entirely separated from each other; on the ganglionic body itself they will frequently appear in the form of the above-mentioned groups. For this reason, it becomes a matter of importance that the material to be used be removed from the body as soon as possible after death, in order to be either freshly examined or to be directly put into a solution of chromic acid. As regards the nervous tissues of man, I have often observed that the structure of those taken from old individuals, or from such as have succumbed to tedious, consumptive diseases, appears paler and more indistinct, than from others, whose death was due to some accident or to some acute disease.*

In the preceding pages I have endeavoured to demonstrate briefly the granular-fibrillous structure of the axis cylinders, and

* Since this paper was written, I have made some examinations of the nervous tissues of the *Amphiuma means*. This animal, remarkable for the enormous size of its blood corpuscles, having a diameter of about $\frac{1}{180}$ mm. in length by $\frac{1}{170}$ mm. in breadth—almost three times as large as that of the blood corpuscles of the frog—becomes further an object of interest with regard to the primitive character of its nervous elements, particularly the ganglionic bodies of the spinal marrow and brain, the simple construction of which goes to corroborate the correctness of my observations. On the ganglionic bodies of the spinal marrow, the plexiform arrangement of their nervous fibrils, which, after departing from them, give rise to the axis cylinders of the nerve fibres, cannot be mistaken; this can be the more readily seen, as they are placed very loosely alongside of each other, with large inter-fibrillous spaces between them. Frequently even, some of them are seen to deviate from their parallel course, and to run in a slightly oblique direction, either upon or below their neighbouring fibrillæ. The granular character of the fibrils is so distinctly marked as to make them resemble strings of beads; they can be easily traced over the coarse nucleus which they thus embrace, from one process to the other. On the ganglionic bodies of the brain, the arrangement is still more simple, for they only consist of a dark-bordered nucleus, embraced by a few granular fibrils, which, on leaving the latter, join to form a few fine short processes.

the ganglionic bodies with their processes, such as it appears after death, when carefully and microscopically examined, but have thus far forborne to corroborate the correctness of my observations by adducing certain collateral facts. One of the most important of these is the mode of development of the nervous tissues, which I have carefully studied, step by step through all its stages, in a considerable number of human embryos. * Here, namely, the formation of the fibrils of the axis cylinder is seen to take place by the linear arrangement and mutual connection of small elementary granules. The fibrils, formed in this manner, and representing the entire nerve, lay, separated by numerous granules subserving to the formation of new fibrils, for a period of time side by side. Not earlier than in the embryo of 3½ months does the formation of the individual axis cylinders commence by the separation of these elementary fibrils into minute bundles, which, somewhat later, are surrounded by a delicate sheath.

For a further evidence of the granular-fibrillous structure of the axis cylinder, as well as of the ganglionic bodies and their processes, I may be permitted to point to the observations of *Frommann* and *Grandry*, made on nerve fibres and ganglionic bodies, previously treated with a weak solution of nitrate of silver; the axis cylinders and processes here showed, as is known, for a certain time a distinct transverse striation, until, after a continued action of the light, the specimens assumed a brownish-black colour. It is very obvious in this instance, that the fine transverse striæ represented the intermediate substance, connecting the granules, and in which the metallic deposit first took place,—and that accordingly, this substance must possess in a higher degree the property of decomposing the metallic salt, than the granules, and must also have a different chemical composition. After a continued exposure to light, this property also manifests itself in the granules and causes the colouring of the whole specimen.

In comparing the granular-fibrillous structure of the axis cylinders and processes, as above described, with that of the striated muscular fibres, we can hardly overlook the analogy existing between the two, particularly during the earlier stages of embryonic life. In a tolerably fresh human embryo, about 17 mm. in length, I found these muscular fibres still in their first stage of development, as those furthest advanced only represented narrow bundles, consisting of very distinct granular fibrils. The intermediate substance, connecting the granules, is here, like in the axis cylinders, only sparsely represented, in consequence of which the latter almost touch each other. Not until the intermediate substance becomes more developed in volume, do the characteristic transverse striæ of

* The results of these researches are too extensive to be mentioned here even briefly; they will be made the subject of a separate paper.

these muscular fibres appear, while the granules become more separated from each other. In the embryo of three months, the striation is already quite distinctly seen; at the same time, the granules appear in the form of minute quadrangles. If we take further into consideration that the electro-motor behaviour of the striated muscular fibres in a state of rest, as well as their changes in a state of activity, are similar to those observed on the nerves, it might be presumed that perhaps, between the terminations of the motor-nerve fibres and these muscular fibres, a more intimate relationship exists than is now supposed. And further, if we consider those so-called *sarcous elements* of *Bowman* as the elementary bodies or agents through which the contraction of the muscular fibre is effected, we might, judging from the above-mentioned analogy of structure, also look upon those granules, composing the fibrils of the axis cylinders, as the true *nervous elements—i. e.* those anatomical elements through which the propagation or transmission of the nervous force takes place.

The sheath of the axis cylinder manifests itself on those of the larger nerve fibres, as already mentioned, through a fine double contour; the finest only make an exception. On many of the larger axis cylinders the course of the double contour appears wavy, a phenomenon which presupposes certain dilatations at different places of the sheath, and also a displacement of the granules. Sometimes specimens are met with on which these dilatations extend into regular folds, as in Fig. 21, which, however, may have been produced by a stretching of the sheath during the manipulation.

It remains for me still to demonstrate the manner in which the fibrils of the fibrillous layer of the nerve-medulla arise from the axis cylinder. Already during the first period of these researches, I devoted a considerable part of my attention to this subject, but without obtaining any definite satisfactory results. Frequently in dissecting carefully with fine-pointed needles very thin sections of spinal marrow under the loupe, I would meet with long naked axis cylinders projecting from the torn ends of nerve fibres, on which the fibrillous layer of the nerve-medulla was observed to terminate by gradually approaching their surface. But as we very often meet in the same way with peripheral nerve fibres, the axis cylinders of which have been for a certain distance entirely denuded of their coverings by the manipulation with the points of the needles, I regarded the former specimens in the same light, especially when the axis cylinders projecting from the nerve fibres were of no considerable length. But the observations of a number of these specimens of unusual length, the denuded condition of which could not well be ascribed to the manipulation, induced me finally to regard them as torn axis-cylinder processes of the ganglionic bodies. As regarded the true nature of the mutual connections between the

different enveloping layers and the axis cylinder, I still remained in doubt, as it seemed to me unnatural that such a large number of fibrils, as that of the fibrillous layer, could arise from so small a surface. Finally, nearly three years ago, when composing the first part of this paper, I repeated my examinations and met again in the grey substance of the spinal marrow with a number of those axis-cylinder processes, on which I found my previous observations most satisfactorily confirmed. On these, namely, I observed quite distinctly how the fibrils arose from the sheath of the axis cylinder *by degrees*, until the whole layer was formed (Fig. 22). As, however, the material on which this observation was made, had been lying for some time in a solution of chromic acid, nothing of the tubular membrane could be seen.

In finally summing up the results of my researches regarding the structure of the *double-bordered* nerve fibre, this will be found to consist of the following parts: 1, of the true nerve fibre, the so-called *axis cylinder*, consisting of a bundle of *granular fibrils*, enclosed within a distinct sheath of their own; 2, of a semi-liquid substance, the *medullary layer*, surrounding the axis cylinder; 3, of the *fibrillous layer*, consisting of very fine, delicate and smooth fibrils, and surrounding the medullary layer; and, 4, of the *tubular membrane*, or *external sheath*, a thin, structureless and elastic membrane, enclosing all the other parts. Whether now the thirdly-named part really exists in the living nerve fibre, or whether it is only produced by coagulation, it must be decided by other, more accurate histological researches than those hitherto made.

## V.—*The Theory of Immersion.*

By Rev. S. Leslie Brakey, M.A.

### *Part I.*

If immersion lenses have the superiority over dry lenses which has been ascribed to them, it is essential that we should be able to account for the difference, as this may be a guide to us not to look for improvements in a wrong direction. This is what I propose to investigate up to a certain point in the present paper. The material for it was prepared some time ago, but laid aside because the preliminary controversy about apertures stopped the way, so to speak, for the publication of it. This controversy has several times seemed to be on the eve of settlement, but still, by some curious fatality, broke out afresh, as new correspondents commencing it brought new objections to be answered.

In a certain sense, of course, it may still be said to be un-

settled; that is to say, there are still some persons who profess not to understand it. But it is now, perhaps, as much settled as, by discussion, it ever can be settled. For as soon as a controversy has reached the stage at which epithets begin to do duty for arguments, it is a sign that it is about time to let it cease and be judged on its merits, so far, at least, as any profit to science is concerned. Neither should I myself have now added anything more whatever had I not this ulterior question in view, which in some sense necessitates it. For, while so distinguished an observer as Dr. Woodward still professes to dissent, it might seem unceremonious to pass on without a word to another stage of the question, as assuming the settlement of this. I have, therefore, as a preliminary, to point out why I do so. I will, for this purpose, take a short retrospect of the question in its later stage, not exactly entering on the controversy itself, but only to call attention from a more general point of view to the course it has taken, and the point at which it has now arrived. I therefore deal with it now only as it has been presented by Dr. Woodward.

One of the things which has probably occurred to the mind of nearly every reader is the wonder how it comes that in a case where the conditions are so few and so definite there could be a controversy at all, and a controversy so difficult to terminate. Anyone can see how a discussion might go on for ever about, for example, the origin of evil, or the best government for France, or the birth-place of Homer. But in optics the conditions are not only simple but mathematical. The work is always reducible ultimately to combinations of two or three definite laws, which have the precision of a strictly mathematical form; and a dispute about the result has a kind of resemblance to a dispute as to whether some triangle is three times or only twice as large as some other triangle. In such a case we should conjecture that there must be some simple account of the fact that a difference of opinion could exist at all; as, for example, whether both parties had really been brought to look at the same triangle. Here, too, the reason is similar and equally simple. The controversy, at the point to which it has now been brought, is not on any one subject or question, but on two different questions *alternately*. If it could have been kept to one thing it must necessarily have been brought to an issue long since and ended. The difficulty is not at all to meet the theory of Dr. Woodward but to force him to say what his theory is. This is a very old difficulty, and one which, as Locke tells us, no logic ever yet invented can overcome; because, as he puts it, *you cannot eject a vagrant from his dwelling-house*.

These two different things are, the microscopical objective commonly so called, and the other construction or "machine," put together, not commercially, but, as he himself has informed us, "for the purposes of this controversy." He is required to say,

in plain words, which of these two he is talking of; on which of them has he made up his mind, if I may so express it, to "place his money"? If he takes up the common glass he is met at once by the fact that, brought to the test, it would only give the prophesied limit. Nor is this met by the suggestion that by an extra twist of the collar it might have been "screwed up" a few degrees more. For, where the difference professed to be attainable is so immensely great, to contend about a few degrees *at the enemy's limit* would be virtually to surrender the case before beginning it. So small a margin near the wrong place has a suspicious look of an approaching total disappearanee so soon as the measurements are correctly made; and it was only too plain that to let the issue be staked on this looked far more like pleading his adversary's cause than his own. This ground, therefore, is abandoned as much too dangerous to be pleasant, and the other alternative taken up. For is there not the new machine, with the hemisphere in front, which not only can give, but does give, more than the limit laid down,—a great truth which ought not to have been "overlooked" by Mr. Wenham. Here again he is met by the fact—the historical fact—that it was not overlooked by him, but, on the contrary, fully developed; that it was not only known to him, but put together by him long ago, and put together for the very purpose of showing that in this way we *could* get a wider angle. So this having also failed he passes back to the other case, and so round the circle again.

Here then is the dilemma which Mr. Wenham presents to Dr. Woodward. There are two different glasses; which of the two are you speaking of? Which are we to understand that your theory is meant for? If the common one, then it is not true; if the other one, then it is not new:—choose for yourself on which of the two horns you are to be impaled.

But Dr. Woodward will not choose. He will answer no questions of the kind, and utterly declines to let his hand be forced by any such unpleasant plainness. At the distance of two thousand miles he cannot of course be put figuratively into a witness box and forced to say which of the two he goes for. Even then he would answer no doubt like the chaplain in the *Vicar of Wakefield*, when the squire imagined he could put him in a corner. "Come, Frank," said the squire, "suppose the Church your present mistress, in lawn sleeves, on one hand, and Miss Sophia here on the other, which would you be for?" But Frank was equal to the occasion. "*For both* to be sure," cried the chaplain. Dr. Woodward likewise is for both; with the proviso always that they come on time about. Whichever you are for, he is then for the other one.

This pleasant position, it is scarcely necessary to say, he did not deliberately select. He drifted into it. And the way he drifted into it was this. Appealed to by Mr. Tolles as the highest autho-

rity, he partly was constituted by him and partly constituted himself a kind of arbiter and Court of Final Appeal for both hemispheres. And in this character he proceeds to deliver judgment, having, however, most unluckily as it turned out, postponed his own study of the subject till after sentence was given. The sentence was given with the proper air of moderation and the proper judicial calmness. Both are right and both are wrong. Mr. Wenham right, perhaps, so far as the only case he knew of was concerned; wrong in so far as he overlooked the fact that the new structure of Mr. Tolles could do the thing he denied. And then Mr. W. has the pleasure of receiving some instructions about the properties of this new discovery. Of course it very soon came back to the teachers that this was only his own construction made long ago, published and laid aside. So—the Court had made a mistake. But then what to do? A "mistake from the Chair" is proverbially hard to deal with. Anyone else may be reprimanded or put out of Court, but the Court itself cannot err, any more than the King can do wrong; nor can its sentence once given be reversed. It can only be ignored. So Dr. W. is obliged to ignore it, or to try to keep it ignored as best he can. And this is how it came about that instead of discussing the question he can only afford to fence with it; because at all risks it must now be kept in a mist. It is a pleasant position to be in, and no doubt he likes it very much.

Once only in all the discussion does he allow himself to be not caught exactly, but very nearly caught. And the way he escapes is worth stopping to look at. So far he had committed himself as to say that the common glass if it did not give the wider angle at any rate it could be proved that it *might* do so,—by theory. Then says Mr. Wenham,—thinking now surely to bring him to book,—how is it done? Here is *my* diagram for you to examine; the diagram of a common objective with all its combinations. Here you see is the course of the extreme ray, all through; here is where it stopped at the "critical angle," and cannot get beyond. You say it can;—very well; just draw me a figure tracing the course of such a ray all through; then when we have got *your* diagram we shall see—what we shall see. This certainly looked very like the beginning of the end, and the answer to the proposal was expected here with some curiosity. Would he draw the figure and let himself be caught; or would he pretend not to have noticed the request? The answer came in his last paper (March). He will not draw the diagram. He will not even undertake to say that he knows how it could be drawn; *but*—the next best thing—though he does not know it himself he *knows a man who knows it.* This man is Mr. Tolles. *He* could tell how to do it. But, he hastens to add, it is quite useless even to think of asking him to tell. For it is a trade secret; and he "gets his living by it." We must accept it therefore, but we must accept it not by sight but by faith.

So then this is what we have come to. We are to set aside a scientific principle because Dr. Woodward is sure that Mr. Tolles is sure that he could get over it. To make any commentary on this would be to spoil it. Indeed a practical comment of a very amusing kind was by mere accident supplied immediately. Mr. Tolles had constructed an object-glass which he labelled with the astounding angle of 180°. And not only constructed it, but in an evil hour sold it to an English gentleman, Mr. Crisp, little thinking that in so doing he was selling himself into the hands of the Philistines to be shorn and made sport of. Mr. Crisp lent it to Mr. Wenham, who of course proceeded to test it. We know what came out. The light had got in at the wrong side, and when it was cut off the true angle came out some 70° less than the label. How it could be that the absurdity of supposing such an angle to be even conceivable did not awaken the suspicions of the maker is what will probably never be known. I say *conceivable*, because such an angle necessitates the proportion between the breadth of the front and the focal distance to be not only great but absolutely infinite. I think I am well within the truth in saying that this if not the greatest is certainly the most *ludicrous* error ever published by any optician living or dead. And this is the artist on faith of whose learning we are asked to set aside a law of optics.

I am very well aware that to everything here said, or that can be said by me, Dr. Woodward has a short and easy answer. It is all disposed of in two words—"total ignorance!" It is not even an argument at all; it is only an "effusion." This, no doubt, is one way of answering. Of course, if Dr. Woodward thinks it seemly and consistent with self-respect to come down to this kind of thing, that is chiefly his own affair. But does he think anyone is deceived by it? or that everyone does not see in it the sign, not of one who has gained his cause, but of one who has lost his temper?

It is therefore, I think, now apparent that this controversy has worked itself to a natural end. It may be, perhaps will be, continued by some one still; but it can only be in a verbal or personal way, not to any scientific end. Where everything vanishes as soon as you touch it, to pursue a question farther is only to pursue a shadow; and it is quite useless to go on with a controversy where there is nothing left to controvert.

I proceed therefore now to the theory itself.

(*To be continued.*)

## NEW BOOKS, WITH SHORT NOTICES.

An Introduction to the Study of Practical Histology, for Beginners in Microscopy. By James Tyson, M.D., Lecturer on Microscopy in the University of Pennsylvania, U.S.A. Philadelphia: Lippincott and Co., 1873.—The author of this little work has made a slight mistake in giving it a title. Assuredly anyone who had not seen the volume would imagine that it dealt with questions of microscopy generally, whereas it is limited to the subject of human histology. However, in this branch we may say that Dr. Tyson has done well in giving a brief account, and one intended only for students who are beginning their work, of the methods now most used in the preparation of the various tissues. It is just the book which the general medical student requires, and which will in a very short time make him familiar with the mode of preparing for examination, and then observing such tissues as skin, fat, muscle, tendon, bone, blood, nerves, &c. &c. With reference to the last-named tissue, which is one of the most difficult for a student to prepare, Dr. Tyson gives the following as Dr. Klein's mode of demonstrating the fibrillar structure of the *axis cylinder*. "A piece of fresh nerve is put in common alcohol for a few minutes, and then stained with carmine. It must then be put into absolute alcohol for twenty to thirty minutes, after previously tearing it out somewhat. It is allowed to remain twelve hours or more in oil of turpentine, and then covered in damar varnish, when it will be found that all the nerve fibres are more or less completely deprived of their medullary sheaths. On examination the *axis cylinder* appears in general to consist of a granulous substance, but here and there distinct longitudinal streakings can be recognized." The author of this work studied for some time under Dr. Klein, at the British Institution of London, and Dr. Stricker of Vienna, so that he thereby thoroughly qualified himself for the task which he has so well discharged in the little essay he has published.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*Structure of the Liver.*—M. Ch. Legros has a very able paper, illustrated by a capital plate, in the last number (April) of Robins' 'Journal de l'Anatomie.' He enters at some length upon the different opinions that have been expressed on the subject by British and foreign observers, and then he gives his own remarks. He shows, it seems to us conclusively, if his two drawings can be depended on, that the liver ducts neither expand to include the hepatic cells, nor that the tubes end abruptly. In his opinion—his plates which represent the structure of the liver in a dog bear him out—hepatic ducts enter each lobule and form an immense number of anastomoses in it. But these

anastomoses are quite independent of the hepatic cells, which in fact they surround. This he considers has to do with the secretion of bile. The hepatic cells he terms glycogenic cells, therefore he supposes they have to do with the formation of sugar. But he by no means so clearly explains their connection with the portal circulation.

*The different Characters of Tumours of the Bosom.*—This is too medical a subject for our readers generally, but those of our professional readers will find the paper full of interest. The author discusses the opinions of Velpean, Sir A. Cooper, and Virchow, and he comes to the conclusion that modern ideas tend much more toward our old English surgeon's views than to either of the others. He gives a good classification of the tumours, which is based upon the differences he has observed, both generally and microscopically. The illustrations are good, and they afford fair justification of the author's ideas.—*Journal de l'Anatomie*, No. 2, 1874.

*The Passage of Blood-cells through the Vessel.*—Professor F. C. Donders and Th. W. Engelmann have been studying this point very minutely, and they have given the results of their inquiries for 1873 in a work published in the Dutch. Those who understand that language will do well to read it. As far as we can make out, they appear to have been unable to succeed in finding any aperture through which the white corpuscle passes through the vessel. But then we must recollect that their observations were not made with a binocular, but with a uni-ocular microscope.

## NOTES AND MEMORANDA.

**Infusoria. — Notice to Microscopists.** — Mr. W. Saville Kent, F.L.S., F.R.M.S., &c., being engaged upon a new treatise on the Infusoria, to be shortly published, invites communications from Fellows of the Royal, Quekett, or other Microscopical Societies, and microscopists generally, upon any new or doubtful forms of Infusorial life that may come under their notice. Any record of phenomena not generally known in association with previously-described varieties will be of value, as also local lists of species. Address, Wentworth House, Stoke Newington, London, N. Postal expenses of specimens will be defrayed.

# CORRESPONDENCE.

## Mr. Stodder's Reply to Mr. Wenham.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *March* 13, 1874.

Sir,—Mr. Wenham having "come out of the shell" which concealed his anonymous informant, who "told him that the objective performs best with a cover $\frac{1}{75}$th of an inch thick," refers to Dr. Woodward's letter in the August number of this Journal. This is the authority that I expected that Mr. Wenham would rely on, if he gave any (for I thought it barely possible that he might read the letter again before he replied to my question, and then see that it would be best to retract), but I had no right to assume that that letter was his authority.

Now, Mr. Editor, if you and your readers will refer to the letter, you can see that Dr. Woodward told him nothing of the kind. There is nothing in Dr. Woodward's letter to authorize any such interpretation. Dr. Woodward does say that the objective "performs admirably" with such a cover; but he does not say that it does not perform admirably also with other thickness of cover. The word "best" is an interpolation of Mr. Wenham's, and on that little word turns the whole tirade of Mr. Wenham.

And this is a fair average specimen of all that Mr. Wenham has written about this objective since it was put into his hands.

Personalities were introduced by Mr. Wenham himself, and I commend his remarks and quotation on p. 112 of this volume to his own study, they do not touch me. He reminds me of the boy by the road side throwing stones at the passers by, who when the lash is applied to his back cries out, "You let *me* alone."

Although Mr. Wenham says that my letter in the February number needs no further reply than that, yet he adds a foot-note of reply of twice the length of the text, which I cannot pass without notice. He says, "My explanations concerning this object-glass have brought invective from Mr. Stodder. In his eagerness to attribute dishonesty of intention to me, he overlooks the fact that the extraneous question of performance may be set at rest." I deny and repudiate the charge of "invective," or attribution "of dishonesty of intention" against Mr. Wenham; I also deny that he has made any explanation of his statements about that glass. He has made certain statements of fact as to its performance, which have now been fully contradicted; yet he neither explains or retracts any one of them. He has made numerous—well say mistakes—I will acquit him of "dishonesty of intention"; but let me remind him of a *mot* of Talleyrand, "A blunder is worse than a crime." That the question of the performance of that objective "so rashly sent by Mr. Tolles" is extraneous to the scientific question that Mr. Wenham and others have been discussing is self-evident; but Mr. Wenham must thank himself for unnecessarily introducing it. Having introduced it, and in the anner he did, he must take the consequences. Having attacked

the professional reputation of one whose whole income depends on the remuneration received from the work of his hands, I was bound to defend that reputation to the best of my ability, and I will. Either Mr. Wenham or myself must "come off second best."

Mr. Wenham now asks me to send him the objective again for another trial. This is cool. I will make a substitute proposition. Mr. Wenham may send his twenty-year-old objective here, together with (if he pleases) *any other* that he has made since,* and they shall be as fairly tried with my $\frac{1}{10}$th Tolles, of 1869, as they can be in London, by as capable experts as there are in London, and each one shall report for this Journal. If Mr. Wenham declines that fair offer, I will make another; I will state what my objective *has done*. It has resolved fairly the 19th band of Nobert's test plate. I have seen it (doubt if I could now, my eyes are four years older than then). Dr. Woodward has made a fine, sharply-defined photograph of the *Amphipleura pellucida* with it; that was before it went to London. Now will Mr. Wenham state as explicitly what his twenty-year-old $\frac{1}{8}$th, and any other objective that he has made since, not exceeding in power a true $\frac{1}{10}$th, can do on those two tests? When Mr. Wenham and Dr. Pigott, Dr. Maddox, Mr. McIntire, Dr. Woodward, and myself, all agree about the "Podura scale," then I will accept that for a test object, not before.

I am glad that Mr. Wenham has defined his position, *i.e.* he teaches workmen to make objectives. It seems as if he thought that Mr. Tolles was such a workman, taught by some teacher. If that was his impression, it was a mistake; although he might teach, he never has; and now that his health is such that he has left for a warmer climate to avoid our cold spring weather, not a first-class lens can be made bearing his name, until he returns with restored health.

It seems that Mr. Wenham is not the only one who has misunderstood that unfortunate glass. I find a reference to it in the annual address of the President of the Royal Microscopical Society. "The lens in this instance was properly corrected as a dry lens." How an immersion lens that won't work well dry, could be properly corrected as a dry lens, is not comprehensible. "It may be quite possible that if the lens had been readjusted, so as to give the best image for immersion in balsam, a slightly greater angle might have been obtained; but this would not have been a fair way of making a comparison, *as it is not the mode in which the glass would ever be employed in actual practice.*" Precisely what this meant I do not understand. The only way that I ever heard of for adjusting a glass "in actual practice" is to adjust it for the cover and object in view,—for maximum angular aperture, to search for it.

Believe me yours, &c.,

CHARLES STODDER.

* If Mr. Wenham will himself bring the objectives, I can assure him a most hearty, cordial, and friendly reception from the American microscopists who have so long used and admired his ingenious contrivances for their favourite instrument. I have shaken hands with him across the ocean for some time, now I shall be happy to do so in person. We will not promise that we can teach him anything about objectives, but will promise that we are willing to be taught.

## Mr. Wenham's Reply.

Having received a proof-sheet of the preceding delicate communication, in order to save the time that has been complained of in crossing the Atlantic, I append the few words only that are required in reply.

To gratify Mr. Stodder by making up a defence against any accusations he may choose to bring against me, especially when an attack (like the preceding) consists of nothing else but personal matters (including a choice moral), is quite a needless task for me. If I did not condescend to "explain" in my last brief note to the long rambling epistle of Mr. Stodder, he must thank his own style and the animus with which it was written; for, with its screaming capitals,* it appeared to me as the effusion of a man more frantic than rational. My stand or fall in the controversy depends upon the aperture question, and I can well bear to "take the consequences" and smile at anything that Mr. Stodder can urge, for, judging from his matter, he appears neither qualified or capable of discussing the principle in a scientific sense, and in any other it is useless to argue on an issue that can never be settled by him. It is not therefore necessary for me to take any further heed of his letters.

F. H. Wenham.

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## Mr. Brakey's Reply to Rusticus, jun.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—Your country correspondent has been carried away so fast by his "dialectic" that he has forgotten to put his difficulty; for the "it" of his commentary is a little vague. I fear that in his case the remedy he must look for is to be found, not, as he thinks, in more dialectic, but in a more careful study of the laws of light. It appears, as far as I can gather, that he takes loss of light to be the same thing as loss of angle; and so, because a certain writer is indeed right, "self-evidently," as concerns the total reflexion in one pencil, he imagines he must be counted right also as to the angular magnitude of the other. This is not so. In the two pencils light is lost unequally, one losing by common reflexion, while the other loses more by total reflexion; but the angles themselves remain equal.

It would perhaps hardly be fair to ask any questions as to what connection his first sentence has with his subject; or any reasons he may have for feeling pleased with such a *very* peculiar way of introducing it. Anyone who can read through the *alias* will see that allowances must be made; and if the opinion he has formed generally of my writing is not a flattering one, as indeed it is not, still it was scarcely to be expected he should praise it.

Yours obediently,

S. L. Brakey.

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* See p. 81 'M. M. J.' for February.

## A Reply to Mr. Pillischer.

*To the Editor of the 'Monthly Microscopical Journal.'*

16, Fitzroy Square, W., *April* 16, 1874.

Sir,—I am somewhat astonished at the tone of Mr. Pillischer's letter in the last number of your Journal. First, as to nationality: My authority was a juror at Vienna, and if, as it appears, his information was erroneous, I regret that I should, however innocently, have repeated it. But the fact does not affect my argument, that native British optical talent was *wholly unrepresented* at the Vienna Exhibition. Beyond that the nationality of Mr. Pillischer is a matter of supreme indifference to me:—

"Tros Tyriusve mihi nullo discrimine habetur."

Secondly, as to deep objectives: I perfectly well remember Mr. P. telling me at the Exhibition that he had no higher power than a $\frac{1}{2}$-inch, but that he expected some higher powers. If these subsequently arrived, and were presented to the jury, it was, I submit, "extreme carelessness on his part" not to inform me personally of a fact of which, after his previous statement, I certainly required to be informed.

It now appears that "a series of object-glasses ranging from 4-inch to $\frac{1}{25}$-inch" were submitted to the jury. Be it so. It must, however, be borne in mind that the makers of $\frac{1}{25}$ths are pretty well known, and may probably be reckoned on one's fingers; and I feel confident that the jurors who took charge of that department would have required, before examining a $\frac{1}{25}$th on Mr. Pillischer's behalf, some satisfactory evidence, beyond mere assertion, that it was *bonâ fide his own manufacture.*

That there may be no quibble as to the meaning of a "*bonâ fide* manufacturer," I think I shall carry public opinion with me in defining an individual to be a "*bonâ fide* manufacturer" of whatever is produced in a workshop of his own, by those who work for him at fixed wages, whether for time, or piece-work; *and not otherwise.*

I remain yours faithfully,

Chas. Brooke.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *April* 1, 1874.

F. H. Wenham, Esq., Vice-President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the meeting were voted to the donors.

A paper by Dr. Anthony "On the Structure of a Lepisma Scale" was read by the Secretary, and drawings in illustration were exhibited to the Fellows.

Mr. S. J. McIntire thought, from the appearance of the scale as drawn, that it was not that of the ordinary lepisma. In Sir John Lubbock's book on the Thysanuræ many other kinds were mentioned,

and he believed that there were many found on the Continent which were unknown here. Mr. Ward had recently found one which gave some very pretty effects under the microscope.

Mr. Slack called attention to the portion of Dr. Anthony's paper in which the opinion was expressed that the ribs were on the under side of the scale, and the cross-hatching was upon the upper surface, and asked if Mr. McIntire's observations had led him to the same conclusion.

Mr. McIntire thought that the results of Mr. Beck's experiments quite tended in that direction, and seemed to show that on the upper side there were the cross-markings, and on the side next the insect there were the ribs.

Mr. Slack said it was very desirable to find out, if possible, what sort of lepisma the scales were taken from. In the ordinary lepisma, where the beading was to be seen, it was brought out very plainly by transmitted light. The peculiar markings drawn in illustration of the paper he had tried to find, but had not been able to do so; he had, however, only one slide of lepisma scales, and many of these had ribs which were very wide apart. He would write to Dr. Anthony about it, and ask him if he knew what species the scale he described was taken from.

Mr. McIntire mentioned that he had seen four or five kinds of lepisma scales, and they were very different from each other.

The Chairman thought it was rather an interesting discovery, and it looked almost as if they would have to begin their work upon scales all over again. The ordinary method of obtaining the scales was to lay a glass cover on the back of the insect in order that some of the scales might adhere to it, in which case, of course, the upper side was that which was looked at; they would now have to mount them the opposite way as well. There were, no doubt, different kinds of lepismæ. He had seen one kind in the tropics which was about $\frac{3}{4}$ inch long, and he wondered if the scales of these would be proportionately large.

Mr. McIntire did not think it was necessarily the case that large scales were obtained from large insects; they were sometimes got from very small ones. The different kinds of scales were very different from each other; he could call to mind three, and knew that there was also one from the Cape.

The thanks of the meeting were then unanimously voted to Dr. Anthony for his communication.

Mr. Wenham said that at the previous meeting he had promised to give them a demonstration of his method of measuring the angular apertures of objectives. He then proceeded to do so, and illustrated his remarks by an instrument placed upon the table, and by diagrams drawn upon the black-board. (See pp. 233, 234.)

Mr. Ingpen said that, upon reading Mr. Wenham's letter in the last number of the 'Monthly Microscopical Journal,' it occurred to him that the same method might be employed with advantage to get rid of false light by cutting off all extraneous rays. His favourite Ross $\frac{1}{4}$-inch objective, of 100° aperture, which was extremely good with oblique light, always showed a blaze in the centre of the field of view when used with direct light. He had a cap made to slide upon it with an

aperture a little larger than the field of view with the lowest eye-piece. This could be brought into contact with the covering glass of an object, when it excluded all extraneous rays. The effect was almost like magic; the whole of the false light was got rid of, the silvery or milky appearance of diatoms, usually present when viewed by large-angled objectives, disappeared, and their markings and outlines were beautifully black and sharply defined. He had not yet measured the angle, but thought that it could not be greatly reduced by the cap, for, with the light thrown by an achromatic prism at the greatest obliquity, so that the field was half dark and half light, *Grammatophora subtilissima* was well resolved. He did not, however, care so much for the alteration for diatom work as for getting rid of false light in general observation. The subject was one of great importance, and Mr. Wenham's remarks were most valuable.

Mr. Slack said it seemed that it was rather a mistake to give such a very superfluous quantity of front lens. He had an $\frac{1}{2}$-inch objective made by Thomas Ross, and if he allowed the condenser to throw light upon the whole of the front lens, anyone would be inclined to think, on looking through it, that the object-glass was not worth a penny, but when he allowed the light to strike only upon the necessary part the effect was very fine.

Mr. Wenham then drew a diagram showing how the outer portions of the front lens might with advantage be ground away, and expressed his opinion that this would in every case improve the light. The marginal parts were perfectly useless, and they could be very easily ground off now that the single front had become the universal form.

Mr. Stephenson mentioned that some years ago he had a $\frac{1}{4}$-inch objective by Ross which gave him a good deal of trouble from the same cause, but Mr. Hewitt made a cap for it which perfectly cured it. He had listened with much interest to Mr. Wenham's remarks, and should like to ask how it was that the thickness of the foil was prevented from reducing the aperture of the lens?

Mr. Wenham said that the thickness of the foil was extremely slight, not more than $\frac{1}{1000}$ inch, and it was brought exactly into the focus of the lens, and there it did not cut off any aperture. If it was moved it would show at once, by its edges encroaching upon the field, when any of the oblique rays were being cut off.

Mr. Stephenson inquired if it were placed above the level of the glass, or below?

Mr. Wenham said it was above the level. With a $\frac{1}{12}$ inch objective the separation was about the $\frac{1}{150}$ inch, and it was brought quite to a knife-edge.

Mr. Slack suggested the deposition of a film instead of using foil. It was quite clear to him that Mr. Wenham's observations would lead to a considerable modification of all their ideas of the need of extreme angles if they were to knock off 20, 30, 40, or 50 per cent. of the usually-received measurement.

Mr. Frank Crisp inquired whether the higher powers were not much more reduced than the lower ones?

Mr. Wenham said that the higher the power the greater was the

error arising from the cause indicated, and the more the appearance of aperture was reduced by this system of measurement.

Mr. Stephenson thought this might be accounted for by the thickness of the foil used.

Mr. Wenham said he set the stop so as to include the marginal oblique rays, and by means of a diagram drawn on the black-board showed that he really allowed a superfluously large opening.

Mr. Slack then proposed a vote of thanks to Mr. Wenham for his communication, which was carried unanimously.

Mr. McIntire read a short paper descriptive of the peculiar structure of the proboscis of a lepidopterous insect exhibited under a microscope in the room, and of which drawings were also exhibited. The paper will be found printed at p. 196.

Mr. Wenham, in inviting remarks upon the paper, said that near to his residence there was a quantity of the humming-bird moths, and it occurred to him, whilst hovering over flowers they appeared able to hold on as if by some anchoring process.

Mr. Charles Stewart said that the appearance alluded to in the case of the humming-bird hawk-moth was due to the remarkable power which it had of balancing itself in the air (the same as that possessed by some of the wasps and flies) rather than to any anchoring. It would be very interesting to know what species it belonged to, and, if a South African variety, perhaps it could be identified by comparison with the fine collection of insects from that part which was to be seen at the British Museum. It appeared probable that it belonged to a race which had to live amongst peculiar flowers, and perhaps it was unable to reach the bottom of the corolla, so as to obtain its food in the usual way, and therefore had to take a short cut to it, like the bees had to do in the case of such flowers as the jasmine and antirrhinum. An examination of these flowers would often show the puncture made by the armed proboscis of the bee, who has in this way got out the honey, as it were, by the back-door. He thought it probable that the moth described by Mr. McIntire might have to do the same.

A vote of thanks to Mr. McIntire was carried unanimously.

The meeting was then adjourned to May 6th.

### *Scientific Evening, April 15th.*

The scientific evening, held on the 15th ult., was remarkable for the exhibition of many objects of great interest, such as are seldom seen at ordinary microscopical soirées, where it is found necessary to consult popularity rather than the direct advancement of science. So many instruments and preparations deserve special mention and description, that to do anything like justice to them would require far more space than can now be devoted to that purpose. Amongst the preparations we may, however, particularly distinguish a remarkable series of sections of tumours exhibited by Messrs. R. and J. Beck, which attracted the attention of the physiologists present.

Mr. Loy's, illustrating the anatomy of *Asopus luridus*, fully invited the epithet "wonderful" freely bestowed upon them.

Mr. How's collection of geological photographs for the lantern

were found exceedingly valuable and attractive, ranging from the Laurentian Eozoon through various strata down to the fossils of the Post-pliocene period, including minute structure as displayed by the microscope, as well as large forms. They are admirably adapted for lectures or private exhibition.

Amongst instrumental novelties, Dr. Pigott exhibited a refractometer, briefly mentioned in the list. It is capable of working with extraordinary accuracy, and will be fully described in a communication promised to the Society.

Mr. Lettsom exhibited an immersion $\frac{1}{24}$th by Nobert, a new direct-vision spectroscope, and some very rare objects, which will be found mentioned under his name. Attention may also be called to the novelties shown by Mr. Bevington, Mr. Crouch, and Mr. Swift.

Mr. Curties showed a new species of apterous Ichneumon.

Mr. Ward, scales of new British Lepismæ; and the list will be found to contain novelties and varieties of diatoms, fungi, &c.

The great hall of King's College was lent, with their usual kindness, by the authorities, and Messrs. Baker, How, and Horne and Thornthwaite, obligingly supplied the lamps. The number of Fellows and visitors present was 120, and they highly appreciated the opportunity for quietly seeing and conversing upon the various objects displayed.

Messrs. Beck and Beck: Twelve microscopes and a series of sections of tumours.

Mr. Charles Baker: Microscopic marvels.

Mr. William Bevington: New form of Stephenson's erecting binocular microscope.

Mr. Conrad Cooke: *Corethra plumicornis* (living).

Mr. Frank Crisp: Hoffman's polariscope, and a biaxial crystal; Ahren's binocular microscope, and the thecæ in receptacle of *Hymenophyllum Tunbridgense*, under a Stephenson's binocular microscope.

Mr. John S. Crisp: Acari from water-rat.

Mr. Henry Crouch: New model binocular stand, with new removable sub-stage, leaving the whole space under the stage clear when taken off, and ½-inch of small aperture for the binocular microscope.

Mr. Thomas Curties: A new species of apterous Ichneumon, from Ceylon; not yet named.

Mr. Frederick Fitch: Serpula and *Balanus balanoides* (living).

Dr. W. J. Gray: Ova of chigoe, *Pulex penetrans*.

Mr. How: Section of tooth of *Diplodus* and *Ichthyosaurus* from Coal-measures; and a series of photographs of geological subjects, consisting of groups of fossils, &c., arranged for exhibition by the magic lantern.

Mr. Thomas Howse: Section of gills of *Agaricus campestris*, section of the tubes of *Boletus*, prothallus of sphagnum, and the very curious antheridia of *Azolla*.

Mr. John Ingpen: Globular silex mounted dry, in glycerine, and in balsam.

Mr. W. G. Lettsom: A $\frac{1}{24}$th immersion objective, by Nobert; direct-vision spectroscope, with only one prism; and solution of sulphate of Didymium for showing the bands therewith; small block of

Didymium glass; piece of Erbium glass; jargoon, for bands; ditto heated and not heated, for bands; Turacus feather, for bands.

Mr. W. T. Loy: A series of slides illustrating the anatomy of *Asopus luridus*.

Mr. S. J. McIntire: *Obisium orthodactylum*. She has spun a web round herself in order to be protected whilst the young are brought forth alive.

Mr. Thomas Palmer: *Batrachospermum moniliforme*, mounted in balsam.

Dr. Royston-Pigott: New micro-refractometer for finding refractive index of lenses or plates of glass for mean rays; also as a linear micrometer.

Royal Microscopical Society: The microscope, &c., recently presented to the Society by Mr. Charles Woodward.

Mr. W. W. Reeves: *Syzygites megalocarpus*, the very curious conjugating fungus.

Mr. E. Richards: *Hydractinia*, exhibited with immersion tube in marine aquarium.

Mr. Charles Stewart: Two slides of a heterapod *Firoloides*; one showing pedal ganglia and nerves, the other cerebral ganglia, eyes and ear-sac, &c.

Mr. Swift: His improved portable microscope; Captain Perry's new method of converting the Ross model microscope into the Jackson, Lister, or crane-arm form; and his new achromatic condenser, which answers the purpose of nearly every piece of sub-stage apparatus.

Mr. James Smith: Leaf of *Salvia*, showing oil-glands.

Mr. H. J. Slack: Silica films and beads deposited by hydrofluo silicic acid gas in glycerine and water. The process and description of these curious objects will be described very shortly to the Society.

Mr. Suffolk: Head of gnat in glycerine.

Mr. Amos Topping: Section of liver, stomach, and small intestine of eel; injected fin and gall-bladder of ditto.

Mr. Varley: His father's lever microscope, his vial microscope, diamond lens, and diagrams of *Chara vulgaris*, &c.

Mr. F. H. Ward: Scales of Lepisma; a new British species, showing parallel and radiating markings on the opposite sides of the scale.

Mr. E. Wheeler: *Aulacodiscus sollittanus*, *A. symmetricus*, *A. Kittonii*, and other rare diatoms.

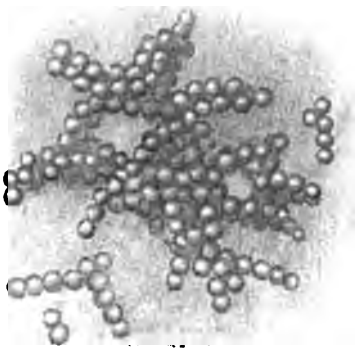
Donations to the Library and Cabinet since March 4, 1874:—

| | From |
|---|---|
| Nature. Weekly .. .. .. .. .. .. .. .. .. .. | *The Editor.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts Journal. Weekly .. .. .. .. .. | *Society.* |
| On the Geometrical Isomorphism of Crystals, &c. By Rev. W. Mitchell, M.A. .. .. .. .. .. .. .. | *Victoria Institute.* |
| Transactions of the Northumberland and Durham Natural History Society. Vol. V. Part I. .. .. .. .. .. | *Society.* |
| One Slide .. .. .. .. .. .. .. .. .. .. .. | *S. J. McIntire, Esq.* |

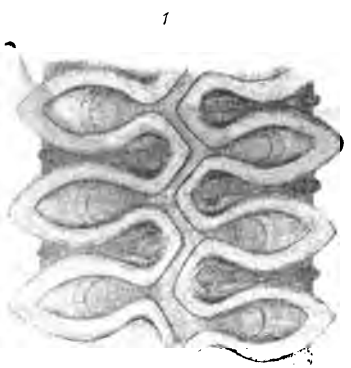
Herbert Horn, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
*Assist. Secretary.*

Beaded Silica Films.



Peritoneal Epithelium.



Respiratory Organs of the Blow-fly.

W. West & Co. lith.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1874.

## I.—*On certain Beaded Silica Films Artificially Formed.*

By HENRY J. SLACK, F.G.S., Sec. R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *May* 6, 1874.)

PLATE LXIII., AND UPPER PART OF LXIV.

ON a former occasion the writer called the attention of the Society to the large number and variety of beaded patterns that could be obtained by making the artificial diatoms of Max Schultze. In forming these objects silicic fluoride gas is allowed to come into contact in its nascent state, with cotton filaments moistened with water. The result is a deposition of silica in the shape of irregular vesicles, the walls of which exhibit beaded structures in definite patterns; the beads in different, and in similar patterns, varying greatly in size; and many of the patterns being composed of two or more sized beads, usually symmetrically arranged.

When the silica in this gaseous state comes into contact with water, in a mass instead of being distributed in finely divided portions adhering to absorbent filaments, it is deposited in amorphous particles, which, when magnified, exhibit numerous flaws, but no regular structure. It seems as if the passage from the gaseous to the solid state was too sudden and violent for any definite pattern to be formed. In like manner when silica is precipitated from its alkaline salts, or "water glasses," dissolved in water, only angular amorphous particles are obtained, and a similar result follows the gradual drying up of a dialyzed solution of silica in water, as the

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DESCRIPTION OF PLATE LXIII., AND UPPER HALF OF PLATE LXIV.

FIGS. 1 and 2.—Cellular aspects of films × with Powell and Lealand's $\frac{1}{8}$th objective.

FIGS. 3, 4, 5.—Organic forms in some films—the framework of the cells probably composed of minute coalescing beads. They were most favourably situated for resolution.

FIG. 6.—Upper figure, branching fungoid-looking threads common on films; lower figure, similar threads resolved into beads.

FIG. 7.—Forms like 6 frequently found on thin films as if sprouting from them, highly magnified with Powell and Lealand's $\frac{1}{16}$th.

FIG. 8.—Remarkable specimen of the same.

The above figures are copied from drawings of the objects made by Dr. Anthony, F.R.M.S., and kindly presented to the author.

films, or nodules, thus obtained easily separate by friction into the same sort of particles.

To obtain any silica deposition of a structural character, it was thought necessary to place some retarding influence in the way of the precipitation of the mineral from the gaseous combination above mentioned, and for this purpose a mixture of glycerine and water was tried and found to answer. The materials for making the gas, powdered glass, or flint, powdered fluor spar and sulphuric acid, were shaken up in a small flask supplied with a glass exit-pipe about $\frac{3}{16}$" in diameter. This apparatus was placed upon a retort stand, so that it was easy to hold an evaporating dish in the right hand and immerse the end of the exit-pipe in the glycerine and water it contained, while the left hand was at liberty to regulate the heat and rate of formation of the gas by approximating or withdrawing a spirit lamp from beneath the flask, as the case required. A little mercury was placed at the bottom of the evaporating dish in order that the end of the exit-pipe might be dipped under its surface, if at any time the deposition of silica became so rapid as to threaten to stop it up. By allowing the gas to come over at a moderate rate, and letting the tip of the exit-pipe dip just under the surface of the water and glycerine, a number of thin films were quickly formed, some of great tenuity, and others thicker, but all avoiding anything like aggregation into lumps.

These films were easily taken up by a small sable brush, placed on a glass slide and washed to remove the glycerine, with successive drops and small streams of water. In doing this, care is necessary not to let the water-stream float them away, or create confusion by carrying one film on the top of another. When freed from glycerine the films can be examined in clean water under glass, or allowed to dry perfectly and then mounted in balsam, the latter being the most convenient. A slide thus prepared will probably contain films of different thicknesses, some perhaps consisting of a single or double layer of beads evenly and closely arranged, and others having more bead layers, and the upper ones either in confused groups or in approximately regular patterns. If the films are in either of the last conditions, transparent illumination with oblique light often exhibits nothing but confusion, and with small pencils of direct light, obtained with the smaller stops of a condenser, an organic aspect is frequently noticeable, as if a number of dingy bacteria-like, fungoid, or small bodies were growing upon some base. Pl. LXIII., Fig. 1, gives a good idea of this appearance in the case of a film tending towards a cellular structure. In Fig. 2 the cellular structure is more distinct, as shown in other films. Figs. 3, 4, 5, exhibit simulations of organic cell forms, and might be taken for veritable results of secretions and depositions occurring in the processes of organic life, but the cell hollows are referable to the bursting of minute gas-

bubbles. A little gentle violence will displace some of the bacteria and fungoid-like bead groups. Fig. 6 group shows a spurious fungoid portion, only partially resolved into beads; another portion perfectly resolved. Figs. 7 and 8, remarkable forms seen with $\frac{1}{16}$th.

I am indebted for the beautiful drawings of these objects illustrating this paper to Dr. Anthony, whose skill both as an observer and artist is well known to and highly appreciated by his brother Fellows of this Society.

The size of the silica spherules forming the films varies, but less so than might at first be supposed, as only those that are favourably situated and carefully illuminated can be distinctly resolved and separated from their neighbours. In two slides Dr. Anthony found most to be about $\frac{1}{30000}$" in diameter, and some close upon $\frac{1}{100000}$". In a slide sent to Dr. Pigott, he found them to vary from between $\frac{1}{40000}$" to $\frac{1}{100000}$", and "had no doubt many by proper illumination can be seen much less than this." The real size of the beads probably varies according to the rate at which the gas is evolved and decomposed by contact with the glycerine and water, and it is the writer's opinion that the smallest beads are considerably less than $\frac{1}{100000}$".

By gently rubbing the films with water in a small agate mortar, avoiding any force likely to pulverize the surface of the mortar itself, they may be separated into myriads of beads, and these with a little washing and subsiding on a slide, may be obtained in nebulous clouds, or more widely scattered. Multitudes of beads in this condition appear excessively minute, and so do those in the thinnest and most transparent films. If such beads are examined with a series of fine objectives, the observer will consider them smaller pretty nearly as it is the theoretical duty of his objective to make them bigger. Thus operating with $\frac{1}{5}$th of Beck, $\frac{1}{8}$th of Powell and Lealand (estimated somewhat too low), and $\frac{1}{24}$th of Nobert (estimated too high), the beads were far from increasing in proportionate apparent size as the magnification passed from between 200 and 300 to between 2000 and 3000, according to the objectives and eyepieces employed, and the strongest conviction of their real minuteness was obtained with the highest power. No glass can give the real size of minute highly-refractive lenses, but experiments with these beads led to the belief that with glasses of fine construction those of the highest power have least proportionate error, contrary to what might have been supposed, and yet quite in accordance with the experience of those who employ objectives up to $\frac{1}{50}$th for very minute researches, instead of relying upon a dictum once current that $\frac{1}{12}$ of extreme aperture would suffice for any magnification required.

These results appear to be in accordance with calculations made by Mr. Lister, showing that certain proportions ought to exist

between the focal length and angular aperture of objectives for their best performance. Thus the best performance is not gained by giving an $\frac{1}{8}$th or $\frac{1}{12}$th as much aperture as works well with $\frac{1}{16}$ths, $\frac{1}{25}$ths, and $\frac{1}{50}$ths; and glasses with excessive angles may be found to exaggerate the size of refractive spherules to a considerable extent. When spherules do not absolutely touch, the best glasses magnify the interspaces more than the diameters of the spheres.

The best way of commencing the study of these films is by viewing them with dark-ground illumination, say with a fine $\frac{1}{2}$ inch of moderate angle (or if of large angle reduced by a stop) and a good achromatic condenser of considerable angle and largest central stop. Some films composed of a plurality of bead layers will then look like fine specimens of point lace, and opaque white. The bacteria-like and fungoid simulating formations that appear dingy brown, with transparent illumination come out like porcelain beads. Formations as in Fig. 2 look somewhat like portions of the moon's surface, and clouds of detached beads give quite astronomical effects, like nebulæ, or portions of the Milky Way.

When a high power is employed on thin films having beads on more than one plane, the colours long since recognized by Mr. Wenham and others as indicating resolution by the best glasses will be observed. Some of these effects are extremely beautiful and polychromatic. Upon these appearances in the slide Dr. Pigott remarks in a letter to the writer, "At the best correction, when the focal point of some beads is brightest and whitest, several coloured beads appear, all on the same plane being of the same colour. The colours vary from red, orange red, yellow, to blue on different planes. If the glass is over-corrected by opening the screw-collar, so as to throw the diffraction rings above, then the colours disappear, and the beading appears of a dull black; but when under-corrected in colour, as the phrase is, the blackness disappears. It is only when the glass is over-corrected spherically that the colours vanish."

With Mr. Wenham's apparatus for dark-ground illumination with high powers these beautiful effects may be obtained. Carefully used, this apparatus gives a valuable light-ground illumination with such a glass as Nobert's $\frac{1}{24}$th, resolving the thin films beautifully and making the beads look very small in proportion to the power. With a fine $\frac{1}{8}$th and C or D eye-piece the illumination becomes dark ground in character, and by focussing just above isolated beads numerous brilliant chromatic diffraction rings may be obtained, each pair of coloured rings being separated by a black ring.

It would be interesting to inquire how far a regulated deposit in minute beads is an approximation towards a crystalline formation, as it is *in form* an approximation towards organic structure. Modern physicists recognize gradations instead of abrupt transitions between

the various states of matter, such as gaseous fluid, viscous and solid, of various degrees of density. When the silica passes from the gaseous to the amorphous solid state, it may be that the rushing and clashing together of molecules is too confused for crystallization, or for the regularity required in organic patterns. A certain measured and rhythmical retardation may be necessary for the formation of crystals, or organic patterns; and in the case of these silica films the violent reaction between the silicic fluoride and water is moderated by the glycerine. No one supposes he is making diatoms by Schultze's process, nor organic cells by the method now described; but some light may be thrown upon processes that do belong to organic life, by noticing how the presence of certain retarding elements may cause crystalline or other forces of aggregation to deposit matter in the shape and quantity required for the structure of living beings.

In some of the slides minute crystals have been found, mostly isolated prismatic needles, or radiating groups of them; also some rhomboid tables, differing little from square forms. In one case a small octohedral column with a flat top was seen. All the thicker films act slightly on polarized light, and so do most of the crystals, but not all, probably from some not being in the right position. These crystals are most likely silicates of minute portions of alkalies accidentally present. Their fewness and extreme smallness would render chemical analysis impracticable. In viewing them with polarized light it is advisable to place the polarizer underneath the achromatic condenser, and observe them with $\frac{1}{5}$th and higher powers.

## II.—*The Suctorial Organs of the Blow-fly.*

By John Anthony, M.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *May* 6, 1874.)

Plate LXIV. (Lower portion).

In bringing before the Society the results of careful investigations into the more minute structure of the suctorial organs of *Musca carnaria*, I have nothing that I am aware of in antagonism with the excellent and conscientious descriptions of the parts of the insect by Mr. Lowne and Mr. Suffolk; I have rather striven through numerous dissections, and the use of the best modern optical appliances, to carry our knowledge a step or two in advance with respect to the mechanism of one of the most curious structures in the insect world.

The "tongue of the fly" has always been one of the most popular of microscopic objects, and the professional preparer of the well-known slides has taxed all his ingenuity to squeeze the bulbous fleshy mass of the tongue quite flat, in order to get all detail into the same focal plane. Worse than that, by the use of Canada balsam as a medium in which to mount the parts, there has been an obscuration or obliteration of the major part of the delicate detail which it will be my business to describe; and I cannot begin my business better than by warning anyone who wishes for the real structure of the "fly's tongue," that one of the commercial slides bearing that label is about one of the most unlikely places to find it. My examinations of the suctorial organs of the fly were carried on for many months—in fact while flesh-flies were "in season," and I dissected a very large number ere I could satisfy myself that the views I was gradually led to adopt with respect to the mechanism of the tubes were not fanciful. For the dissections I employed the special "Dissecting Microscope" of Smith and Beck—a modification of that of Oberhäuser, affording, like it, a varying amplification from two diameters to about 120, but having the advantage of a special correction in its ⅔rds objective for equalizing the definition through this large range of powers. Long practice on minute objects enabled me easily, by aid of a couple of needles, with a suitable power of this instrument, to take the fleshy lobes of the fly's tongue, to separate the component parts, and to arrange them for careful examination in the usual compound microscope kept always ready in position, armed with the most modern objectives, and carefully corrected in every way for errors arising from refraction or faulty illumination. The objective I principally depended upon was a fine ⅛th of Messrs. Powell and Lealand, and this of course used for transmitted light; but I took care at the same time, so far as I was able, to view the same structure by reflected light—with of

course no covering glass. This required some management, as when quite wet the object would present only a glare of light, and when dry the parts would be shrivelled; but by carefully choosing the proper moment when surface evaporation had taken place, a very satisfactory view could be got with a ⅛th objective, but many a hundred coaxings were necessary to get the parts into definite positions ere I could satisfy myself of the arrangement of what I will venture to call the "suckers" attached to the pseudo-tracheæ of Diptera; for, so far as I have been able to make out, there is a certain identity of arrangement for suctorial purposes in the tongues of all the so-called flies, though the shape of the lobes and the arrangement of the teeth may and do differ very materially.

The general appearance and arrangement of the pseudo-tracheæ, I take it, are well known to microscopists; these quasi-tubes have been called "probosces," from a certain resemblance they have to the trunk of the elephant; but if I am correct in my observations, these *insect* probosces have the advantage over the *animal*, in that they can take in fluid not only at the distal ends, but also, at the will of the creature, along the whole length of the tube. If we look carefully at a tube or proboscis, we shall find down the said whole length a zigzag slit or furrow, which is kept open by a series of incomplete chitinous rings, first described, I believe, by my friend Mr. George Hunt, each incomplete ring having at the ends a quasi-point and a crescent, which are opposite to each other, and form the framework of the fissure; and the point clothed with investing membranes projecting opposite to the hollow of the crescent gives the zigzag effect, which can easily be seen on examining by reflected light the lobes of the tongue of a freshly-killed fly. At the same time, it will be observed that these pseudo-tracheæ are imbedded in the substance of the fleshy lobe, so that the edges of the fissure, and the "ear-like" appendages I am about to describe, project but little above the general surface of the lobe. I call these "ear-like" appendages, because in certain profile views of a pseudo-trachea—and particularly when the object has been squeezed down and deformed—a couple of ear-like bodies will be apparent as in some way connected with the crescentic end of the chitinous ring or arch. Now, seen with as little disturbance of the parts as may be, and viewed directly in front, the membranes which form this spurious appearance of "mouse's ears" or "bat's ears," will be seen to be arranged as I have drawn them in Fig. 1, each membrane attached round the edges of the crescents in a way that will be more readily understood by an inspection of Fig. 2, where the parts are seen in profile, a copy of an existing preparation, one out of many dissections, where, by cleaning away the matter, whatever that may be, from the back of the lobe, and by careful tilting, the fragment of the tube has been got into the position the pseudo-

trachea would probably occupy when in preparation for the exercise of its suctorial functions; and I do not think I strain probabilities in believing that these membranous expansions attached to the chitinous crescents, arranged in a double row on each proboscis or pseudo-trachea, may be taken to be suckers, either for the adhesion of the fleshy tongue, or for the imbibition of fluids, or perhaps for both purposes. The view particularly favouring this is well seen in Fig. 2, where a little of the interior of the tube is visible. I take it that by this arrangement, shown in the drawing, each sucker, opening into the tube, and supported by the chitinous crescent, can be applied to any flattish surface, and can be made to serve, as I said before, either for imbibition, or simply for the purpose of adhesion.

I am disposed to think that, looking at these figures, which I have drawn most carefully and conscientiously from preparations, the majority of which I still possess, that the chitinous rings form a very important part of the mechanism of the suctorial parts of the fly, as these, in the quiescent state of the tongue, by their elasticity, keep the fissure open, and at the same time keep what I take to be the suckers ready for instant action. These rings, or, as they should be called, arches, are imbedded in a fleshy material, which I fancy to be principally muscular, which, brought into action, would bend the chitinous arches till their extremities would be in apposition, when the longitudinal fissure would thus be closed, and only a series of openings left from the suckers into the pseudo-trachea through the crescent portions.

Assuming the elasticity of these chitinous rings as playing a part, then the operation of sucking with the tongue applied to any surface might be thus described.

The fleshy lobes of the tongue being forced into close contact with the said surface, the same muscular pressure round the chitinous rings would diminish the calibre of the pseudo-trachea, make it into a tube by closing the longitudinal fissure, and bring the bell-like mouths of what I regard as principally the organs of adhesion into the position and semblance of so many cupping glasses. So arranged, I take it that the relaxation of muscular effort would, by allowing of the resiliency of the chitinous rings, cause a vacuum in the tube, and set up a pumping process; and by alternate muscular action, fluid in the pseudo-trachese would be forced into the œsophagus, while the same pressure would make the adhesion more perfect.

This may appear fanciful. I could hardly claim it as meeting all possible conditions; but so far as it goes, it seems to me fair to assume it as being in accordance with well-known physical laws, and the appendages I have figured and attempted to describe appearing to have an analogy with the sucking or rather sucker-

like organs of Cephalopods, Echini, and certain insects. That there is frequently a very close application of the tongue of the fly and a seemingly well-marked effort of suction, will, I think, be readily accorded by anyone who has experienced the sensation immediately following the alighting of a fly upon his forehead.

The weak point of this theory would seem to be its apparently requiring for the development of the suctorial power a close pressure of the so-called tongue on a tolerably flat surface, and as apparently requiring us to overlook the evident power of the insect to absorb fluids, such as milk and syrup, by some simple process of aspiration. But even in the latter case it would be fair to assume that such aspiration, where the tongue could be immersed in the fluid, might obey the same laws as in the purely haustellate insects. These speculations—and they are but speculations—may not be confirmed by a more perfect knowledge which the future may open out to us; but they are based on most minute and careful dissections of one of the most interesting and beautiful of microscopic objects, and I have only ventured on the theory in the belief that circumstances in a great measure seemed to warrant it.

## III.—*On the Use of Black Shadow Markings and on a Black Shadow Illuminator.*

By Dr. Royston-Pigott, M.A., F.R.S., &c.

(*Read before the* Royal Microscopical Society, *May* 6, 1874.)

The goodness of a telescope may be very fairly judged by the appearance of what is known to be black shadow markings. It is only natural that the same method should be applied to objects observed by the microscope.

But it is impossible to produce these in all their beautiful intensity either with a bad magnifier or a bad illuminator.

The bad glass cannot show them if there, but the bad illuminator cannot produce them.

Cross rays cause as many shadows as there are directions. The shadow of an object illuminated from different directions produces a complex shadow of great intensity totally differing from the original. Thus if you hold up your hand and throw shadows from several candles, only the shadow common to each will be black on white paper, and this will bear little or no resemblance to the original.

It is known that parallel rays give the clearest shadows, but for minute objects even a divergent pencil will give a finely formed shadow; or a convergent one provided there is only one origin of light; the penumbra is reduced as the size of the origin is diminished; but the most convenient of all for black shadow illuminations is the method of using an obliquely placed objective described by me,* and now illustrated by the instrument sent for inspection to be described farther on.

Employing the other day some very old port wine as an immersion fluid, I was surprised to see some extremely minute monads rolling and careering in the fluid more intensely defined than ever. The black shadow illuminator produced in each an intensely black appearance, which rendered their rotation and movements unusually distinct.

* Dec., 1869, 'M. M. J.' "Aplanatic pencil of rays, axis inclined from 15° to 20°." Sollitt had long before used obliquely placed condensing lenses, but these were not *aplanatic*. I have used this method at least ten years, employing inclined object-glasses of various powers. The Ross 1/4, a very fine glass, answers admirably. Messrs. Powell and Lealand in 1862 constructed for me a large *semicircular arc* attached by swivels underneath the stage so as to admit illumination by extremely oblique rays. I then applied as condensers inverted variously stopped off Huyghenian eye-pieces (achromatic, as generally supposed), and next constructed a sub-stage within the main-stage carrying a gimbal with rectangular motions, into which was introduced an old inch, and sometimes a half-inch of Powell's; these were adapted in 1863. I dare say the same idea has occurred to and been executed by others. Some years after this Dr. Matthews, I believe, advocated the same plan. (I showed this stage to Powell and Lealand shortly after I had made it.)

The cause of this black shadow is not without interest, and it depends upon the following principle.

If we observe an isolated spherule of Mr. Slack's new silica slide, some very beautiful and instructive phenomena present themselves.

(1) *Large spherules* show a beautiful miniature image of the lamp flame above; and below it out of focus, a black ring or boundary of the spherule with a faint diffused light over the spherule and around it.

(2) *Small spherules* give a round disk of light above, and below, a dark boundary or circular ring, surrounded with a brightish halo.

(3) *Still smaller spherules* give a round disk of light; and at a certain stage the disk remains the same in size, notwithstanding the spherules are taken smaller and smaller.

Now a black shadow of an oval shape is immediately developed by accurate side illumination of the purest kind, *viz.* by a low-power object-glass free from aberration. The intensity of these shadows can be brought out by the ordinary mirror, or prism.

[If you try to get an image of the lamp from a mirror you will find at one place the flame is a white stripe, and at a different distance it becomes a larger stripe at right angles to its former position.]

If a diatom be employed (large in proportion to the poorness of the glass), the black shadow may be seen of a crescentic form; and by revolving the stage this black crescentic shadow may be made to revolve, so that every part of the spherule is equally capable of producing the same shadow, the finest proof we can possibly conceive of sphericity in a refracting particle.

If a row of spherules be thus illuminated and observed feebly, the individual shadows degenerate into a notched dark line; still more feebly (as by an instrument unequal to its task) into a blurred thick dark line.

[Just the same as we used to see the *formosum* and *angulatum* with the belauded glasses of days gone by.]

Mr. Slack's slide exhibited thus with the highest delicacy of definition at our command, develops a mass of oval black shadows as intense as ink upon snow, every one produced by its corresponding bead.

The exclamation markings of the Podura disappear at once by this treatment, which could not happen if they were realities.

They are replaced by long ribs and double black shadows, when viewed with the long axis of the scale placed perpendicular to the direction in which they are illuminated.

Besides, in the larger sort every one of its component spherules exhibit the same rotating black shadow as Mr. Slack's silica beads

by similar treatment; many of them also give the black boundary ring, and the white disk above the plane of that ring.

If we remember how exceedingly colourless is a film of black ink the 50,000th of an inch thick (placed between two glasses), we can at once understand that a minute spherule of organic matter may naturally be also colourless, and be capable of forming a spurious disk: in no case have I ever been successful in forming a solar disk less than the 60,000th of an inch in diameter under the microscope; although theoretically it ought to have been optically a miniature of a millionth of an inch.

Bright lights will always form large disks of minute refracting spherules, and this is often a source of much obscurity.

In viewing the intercostal spaces of Podura, which are generally *blank* in most microscopes, or at best in the largest specimens cross-barred, I have found a test of correction of the greatest value to render these spaces brighter and brighter by the screw-collar and searcher, when at last I have been rewarded with the development of a rich beaded structure in these blank intercostal spaces. *But* in all cases the so-called spines are found in a focal plane between the upper and lower set, Mr. R. Beck's "out of focus and out of adjustment" ribs engraved on his plate being a real approximation to true structure. The black shadow illuminator first constructed by me in 1864, finely develops the spherical shadow-phenomena.*

The instrument consists of a strong brass slide for the sub-stage, mounted very firmly with an axis carrying an object-glass. A Ross' 1½-inch with a shield is generally preferred. The angle of inclination employed is generally about 15° or 20°.

The wide-angled achromatic condensers are so egregiously full of spherical aberration as to be incapable without stops (and even with them) of producing pure shadows jetty black so necessary for delicate investigations.

When a pinhole stop is placed above these wide-angled condensers, the cap comes so close to the slide as to endanger the glasses of high powers. The angular aperture is then reduced to about 10°. Any object-glass such as an inch stopped off behind or before produces precisely the same effect with the double advantage of superior aplanatism and a safe convenient working distance.

* The highest degree of shadow is produced by stopping off exactly one-half of the front lens of the illuminator, *viz.* that half which is nearest to the axis of the microscope, and in some cases a U-shaped aperture greatly heightens the effect, the top of the U being turned away from the axis.

## IV.—*The Theory of Immersion.*

By Rev. S. Leslie Brakey, M.A.

### *Part II.*

The essential part of the question remains,—to determine in what way we are to explain the higher definition of immersion lenses? It is not to be explained by difference of angle, because in glasses as hitherto constructed the angles are the same. Can it be accounted for by the difference in the loss of light by all the reflexions at the bounding surfaces of the media? This opinion has been expressed in a general way by Mr. Wenham, and it is easy to see at once that the amount of such losses is a thing which from its nature admits of exact calculation, so that we can provide ourselves theoretically with a test how far the cause assigned is an adequate cause. This is what I propose to do in the present paper, with the view of course, ultimately, of having the results which follow from theory compared with those which may be found experimentally.

I must, however, premise that in this I am going on the assumption that the immersion lenses do actually possess the superiority of definition which has lately been ascribed to them. Whether this assumption is justified by the facts is a question on which I would rather not pronounce an opinion. I confess that I have not myself made out that strong, clear, and unmistakable difference which so many observers profess to see; at least, I think I should not have made it out, if I had not so often been told it was there. Where we are convinced beforehand by a *consensus* of authority that such or such ought to be our perceptions, we are apt to fancy we do perceive what we ought to perceive, as in judging the merits of wine; and I have never been able to discriminate exactly how much of the difference I saw was really due to my eyesight and how much to my belief. And perhaps a good many others, if the truth were known to themselves, would find that something of what they profess to see so clearly is likewise due to their convictions as much as to their eyes. Nevertheless there is certainly of later times so decided a balance of opinion expressed in favour of these, by observers who seem to have worked carefully with them, that I think it may, for the present at any rate, be assumed that there is in some way and to some extent a superiority actually ascertained, and which may be relied upon.

We have to see, then, how far it may be explained by the loss of light at the different surfaces, so as to get the amount finally transmitted in all the different cases into the front of the object-glass. Beyond this, of course, we have no business to follow them for the purposes of comparison in *this* inquiry, because their sub-

sequent treatment at the different combinations is much the same for all.

A ray of light incident on the limiting surface of two media is partly transmitted and partly reflected. And if the proportion of the resulting intensity were always the same, our work would be easy enough; the effective light would in every case be given at once by the versed sine of half the aperture. But the proportion varies with every change, both in the nature of the media and the obliquity of the incidence. The change in the media is definite and easy, but the change in the obliquity advances by infinitesimal degrees. So that to get at the total resulting amount we must have recourse to systematic calculation. The *results* are easily intelligible to everyone, and for purposes of experiment and testing require nothing more difficult than to understand the meaning of the word angle. But the computation itself is not of a kind which the majority of the readers of this Journal would feel any interest in following; and from the general nature of the communications which usually appear it would seem, I think, out of place to fill up the pages with the details of arithmetical or mathematical work. Anyone who wishes to test it can always fill in these details for himself. But inasmuch as any value there may be in the investigation turns entirely on its truth, I will first give in outline the method which I have followed, and for which I will ask only a single page; so that anyone so disposed may verify the work at his convenience.

A ray being incident at an obliquity $\theta$ and refracted to an obliquity $\theta'$, the intensity, I, of the transmitted light is

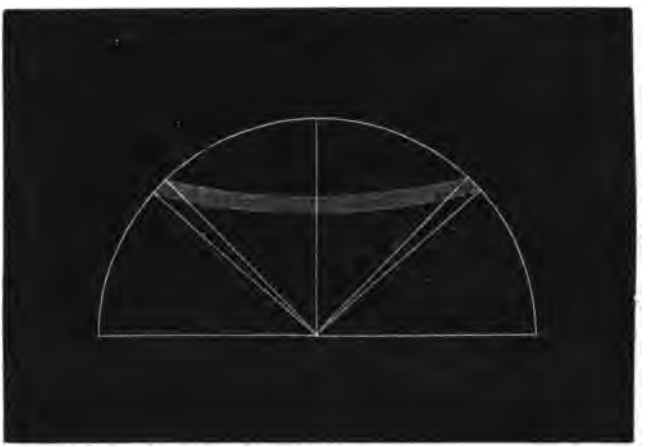
$$I = \frac{1}{2}\left[\frac{\sin.^2(\theta+\theta') - \sin.^2(\theta-\theta')}{\sin.^2(\theta+\theta')}\right] + \frac{1}{2}\left[\frac{\tan.^2(\theta+\theta') - \tan.^2(\theta-\theta')}{\tan.^2(\theta+\theta')}\right],$$

one of the two terms giving the intensity of the light polarized in the plane of incidence, and the other that polarized in the perpendicular plane. If we take the whole emitted light, corresponding to a hemisphere, as our standard, or unity, then after loss by reflexion at the first surface, the transmitted light of the entire cone corresponding to a semi-aperture $\theta$ will be

$$L = \frac{1}{4\pi}\int_0^{\theta}\int_0^{2\pi}\left\{\frac{\sin.^2(\theta+\theta') - \sin.^2(\theta-\theta')}{\sin.^2(\theta+\theta')} + \frac{\tan.^2(\theta+\theta') - \tan.^2(\theta-\theta')}{\tan.^2(\theta+\theta')}\right\}\sin.\theta\, d\theta\, d\psi,$$

in which $\theta'$ is, of course, an implicit function of $\theta$ and the index of the medium. One of the two integrations is effected instantly; the other, however, cannot unfortunately be got in any shape worth having. To arrive at our second integral, therefore, we are obliged to take the primitive and circuitous route of making it up by finite summation. Let a semicircle be conceived drawn round the source of light; and let this be cut up into zones, as in the figure, by planes

perpendicular to the axis, each zone being thus the part of the hemisphere intercepted between two consecutive right cones whose common axis is the axis of the object-glass. From one of these cones to the other the proportion of light reflected will of course



vary. Let the mean value be taken, and then if the zone be cut thin enough we may without any appreciable error assume the value of the multiplying coefficient which gives the lost light (or the transmitted light) to be constant throughout the zone. In this way, by adding all the zones together we get the whole amount of effective light corresponding to any original semi-aperture $\theta$. At the commencement or near the axis the change is extremely slow; the loss at 20° *e.g.* being very little different from the loss at 0°. But as the angle increases, the change in the coefficient becomes very rapid, and for all the higher apertures the belts must be taken very thin, or an appreciable error would arise.

Taking the sum of these up to any angle we wish to know, we get the whole effective light for that angle, and adding in the intermediate ones we get the value for the next aperture that we wish to record. This must be repeated for all the bounding surfaces. In the case of a dry object we have to reckon with two losses at the surfaces of the cover, and another at the front itself. Using an immersion front, we get likewise three diminutions—one at the under surface of the cover, with the same index as before, and two more with a different index, *i.e.* from the cover into the water, and the water into the front.

Taking, first, the case of an object not in any medium, the results are given in the subjoined table for as many apertures as appear to be of use for the present purpose of comparison.

OBJECT IN AIR.

| Angle of Aperture. | Effective or Transmitted Light. | | |
|---|---|---|---|
| | Without Covering Glass. | With Covering Glass. | With Immersion Front. |
| ° | | | |
| 50 | 90 | 82 | 89 |
| 75 | 198 | 180 | 196 |
| 100 | 340 | 308 | 337 |
| 120 | 472 | 421 | 468 |
| 130 | 541 | 476 | 537 |
| 140 | 610 | 526 | 605 |
| 150 | 676 | 567 | 669 |
| 160 | 734 | 594 | 726 |
| 170 | 777 | 606 | 769 |

All the intermediate apertures from 0° were, of course, necessarily calculated in obtaining these. But the insertion of them does not seem to be practically of use, and might serve to confuse, perhaps, rather than to assist where general results are being looked for. The index for glass is taken at the common value for crown glass, *i. e.* 1·525; for water, 1·336, the relative index being therefore 1·141. And in the tabular form it is necessary to observe that the total emitted light for the full cone of 180° is taken, for convenience of fractions, not as unity, but 1000. The nearest whole numbers only are given. On this scale, then, of 1000 we have the amount of light actually in work for any angle, and can compare it either with a higher angle or with the same angle under different conditions. Above 170° the figures are suppressed intentionally, because for such extreme apertures the measurements are doubtful and uncertain in the highest degree, if indeed such apertures can be said to have a real existence at all. It was, in fact, in attempting to form a judgment of this that I was first led to this calculation; and in continuing it on above 170° we are led to see by theory what may be verified easily in practice, the worthlessness of all professions of exactness in such extreme apertures. They are purely paper angles. And it is foolish and absurd in the highest degree for opticians to profess this extreme exactness in their lists, although they can scarcely be aware of the extent to which the absurdity reaches. One optician has on his list, let us say, the highest angle as 170°; another has 175°; and then, when we read in some other list 176°, we think, of course, that this firm has beaten the last—very little indeed,—by a head and neck only;—but still beaten them. Now what is the fact? Suppose a ray at this obliquity *could* be got at; how much of it could we finally use? In such a case it must pass necessarily through a cover; and if we try, by continuing this table, how much we arrive at as effective, or actually transmitted into the front—what "percentage" *e. g.* of the

ray itself arrives—we find that, omitting fractions, what we actually get for use is *nothing per cent.* The actual proportion is about $\frac{1}{125}$, which as the original ray itself is so small, is a quantity wholly imperceptible.

The other table is for an object mounted in balsam, or any medium of the same kind, the index being here assumed the same as for glass.

OBJECT IN A MEDIUM LIKE BALSAM.

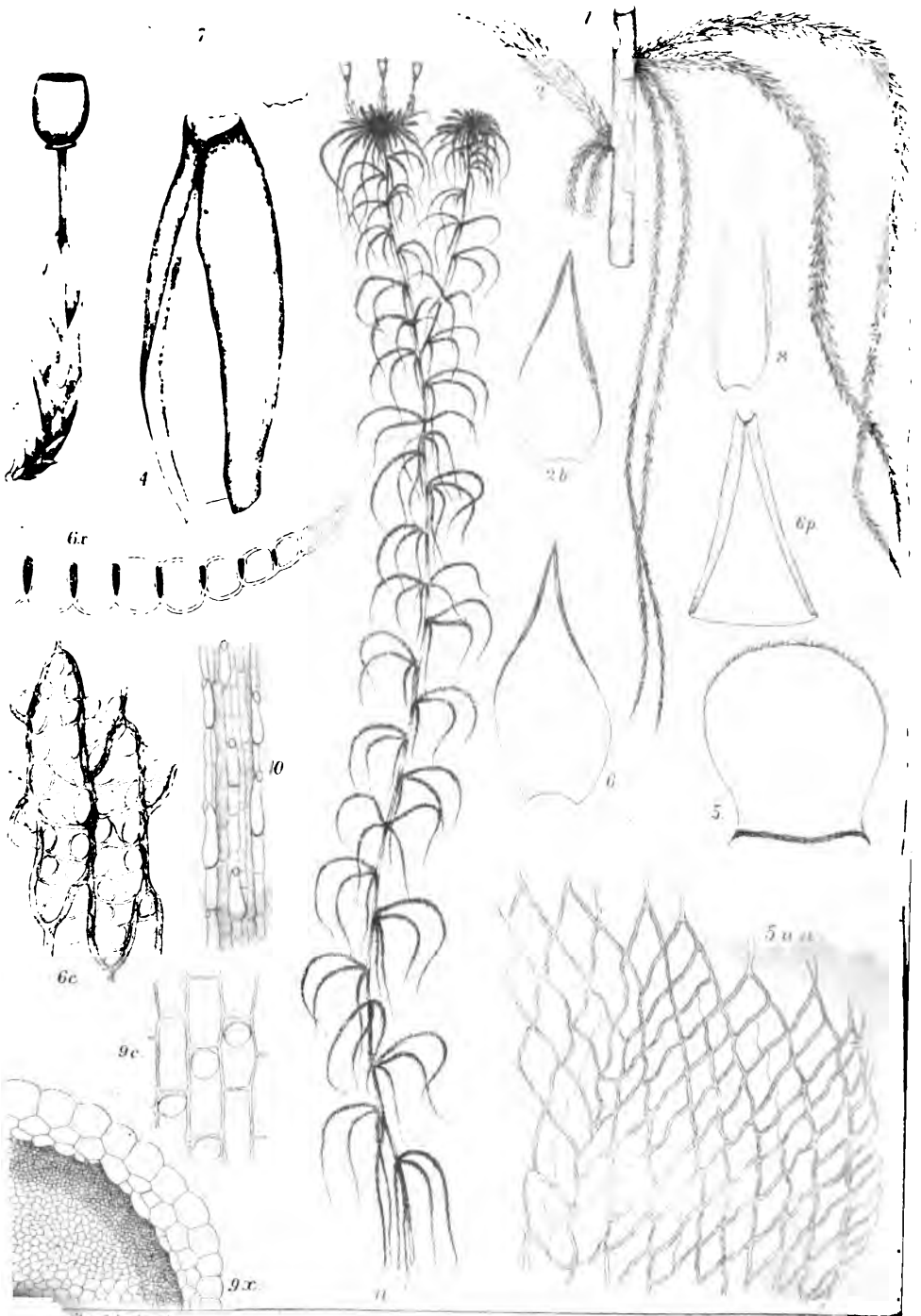
| True Aperture. | Transmitted Light. | | Apparent or Air-Aperture. |
|---|---|---|---|
| | With Common Front. | With Immersion Front. | |
| ° ′ | | | ° |
| 32 10 | 35·8 | 38·8 | 50 |
| 47 4 | 75·99 | 82·5 | 75 |
| 60 18 | 122·7 | 134·1 | 100 |
| 69 12 | 158·4 | 175·2 | 120 |
| 72 56 | 173·5 | 194·0 | 130 |
| 76 4 | 185·5 | 210·3 | 140 |
| 78 36 | 194·0 | 223·9 | 150 |
| 80 26 | 198·8 | 233·9 | 160 |
| 81 34 | 200·5 | 240·2 | 170 |

Two reflexions only are here encountered. The true apertures are given in the first column, but as these would have to be taken by some special method, as by the "tank" or some equivalent arrangement, the corresponding air-apertures are added for convenience. The apertures here selected are not in round numbers, because in that case the apparent apertures could not be expressed in round numbers. But if we are proceeding by steps, it is, of course, of no consequence at what exact places we stop. For uniformity, therefore, the round numbers are still kept in the air-angles, though these, of course, are not the real apertures. And in the table a decimal place is added on account of the smallness of the *differences* in the higher angles.

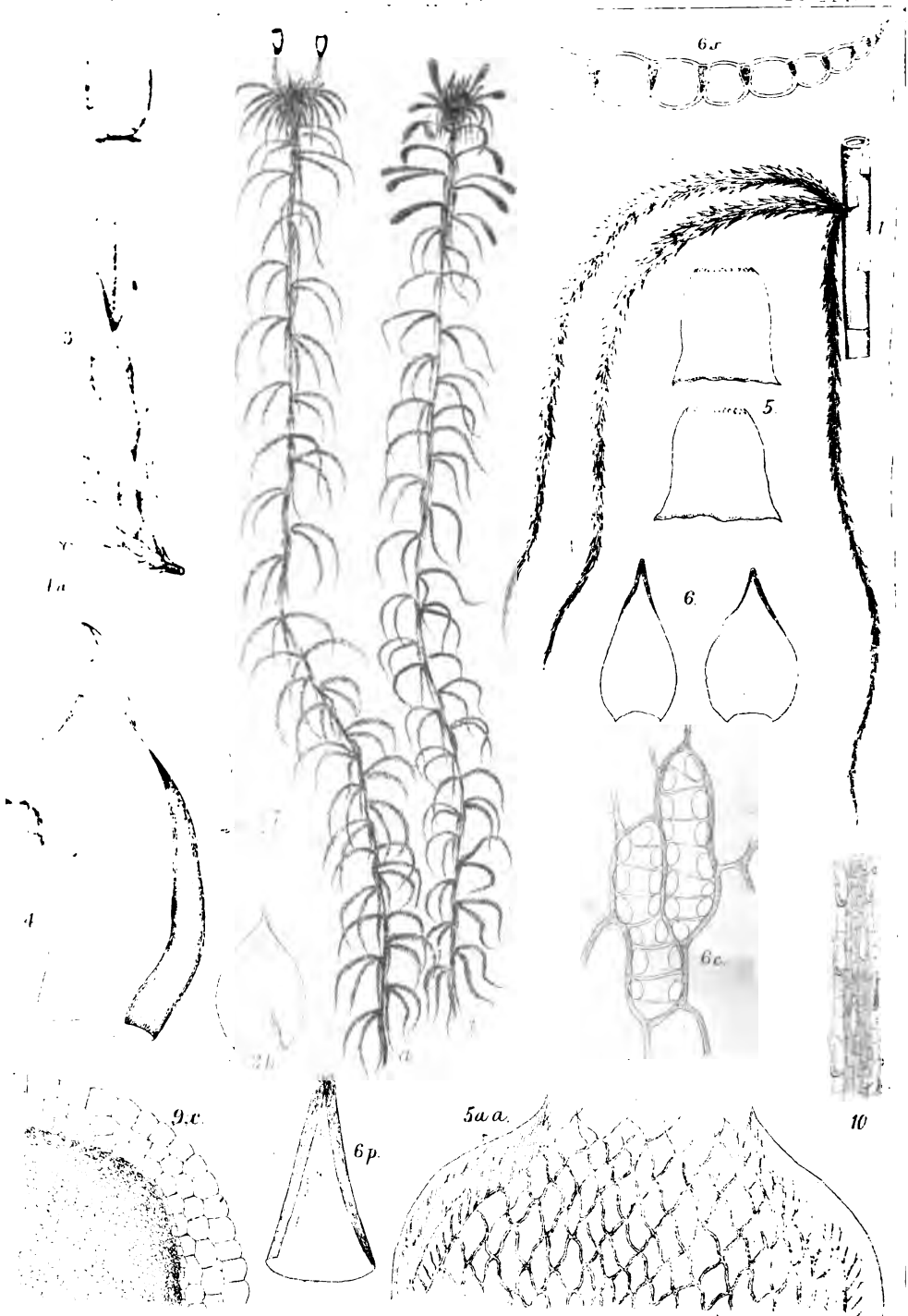
These figures afford us all that is necessary theoretically for testing the question whether the superiority claimed for the immersion glasses is due to the light lost in passing from medium to medium. I do not profess at present absolutely to answer it, partly for the reasons given above, but partly also because the observations must be made in a special way; and my principal purpose is to call the attention of those who may possess the requisite skill and leisure and appliances to the principles upon which, whenever it is answered, the answer must be founded. At first sight there seems to be less difference than we might have expected. But then it is to be observed that the numbers, taken in the form in which they stand, do not give us the necessary tests. They must be subjected to a little manipulation first. Take, for example, as an extreme

case for illustration, the aperture of 170° in the first table. The first column we may here reject entirely as useless in the high angles, because we must use a cover unless we have thick glasses specially constructed. Looking, then, at the other two columns, there does not seem to be a great difference between the numbers 769 and 606; nothing, at least, very startling to lead us to expect much gain. But then this way of comparison by totals may be wrong, or rather is certainly wrong. For in applying to the resolving of tests it may be the more oblique rays that do the resolving work, or most of it. Now let us look, not for the whole gain, but for the gain at the extremity, *e.g.* in the last ten degrees. We may, in fact, suppose the inside of the glass stopped off, all except the interval from 160° to 170°. *Then* to get the proportion we subtract each number from the preceding one in the column, and we find that for *this ring* the immersion has about four times the light of the other. Therefore, to test experimentally where our real strength lies, we must be able to divide our glasses by rings; that is, we must have stops not alone like those usually made, *but* such that we can stop out the inside and preserve any ring of any magnitude we wish. To carry out this comparison by "exclusions and rejections" of course needs skill and patience, *and* appliances. I have attempted it myself on a limited scale, but failed to go on for want of these appliances, and of the necessary mechanical skill. It is only those who are near opticians, and can have their ideas carried out at any time by skilled labour, who can arrive at trustworthy results. It is perhaps worth observing that in such comparisons great care would have to be exercised in determining apertures truly; remembering such things, *e.g.* as the difference we may be bringing in, without thinking, by adding on water and a covering glass which changes the true front or first refracting surface to another one altogether. And other such precautions; for in measuring apertures, notions are still very loose, and results sometimes given which are wholly untrue.

So much is plain, that the cause thus assigned is a *vera causa* in Newton's sense of the phrase; that is to say, the thing assigned as the cause is a really existing thing. And on the face of the calculation it is also clear that *some* difference is truly accounted for by its action. Whether the whole is accounted for by it is the question to the answer of which this investigation supplies only the first of two steps. If on dissecting the apertures for comparison it should be found inadequate, then some other cause will have to be sought for, not instead of this but in addition to it.




Sph. fimbriatum.




Sph. strictum.

## V.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Plates LXV. and LXVI.

### 12. Sphagnum fimbriatum Wilson.

Hook. Flora Antarct. II, p. 398 (1847).

Plate LXV.

Syn.—Wils. Bry. Brit. p. 21, T. LX. (1855).—Schimp. Torfm. p. 59, T. XV. (1858).—Synop. p. 674 (1860).—Lindb. Torfm. No. 4 (1862).—Berkel. Handb. Br. Mosses, p. 307 (1863).—Hartm. Skand. Fl. p. 81.—Russow Torf. p. 51 (1865). Milde Bryol. Siles. p. 386 (1869). Hobk. Synop. Br. Mosses, p. 24 (1873). *Sph. acutifolium* p. p. Hook. and Tayl.—C. Müll. et alior. auct. *Sph. capillifolium* Dozy and Molkenb. Fl. Batav. p. 78.

*Monoicous; in loose pale whitish-green or glaucous-green tufts.* Stem very slender, elongated, 6–14 inches long, pale green, with 2–3 layers of porose cells. Fascicles of 3–4 very long attenuated branches, of which two are arcuate and decurved, 1–2 pendulous, filiform. *Cauline leaves large, erect, broadly obovate and obovate-spathulate, the margin in the rounded upper half laciniate-fimbriate;* hyaline cells of the middle and upper part rhombic, with one or more partitions, and without fibres or pores; *chlorophyll cells long, linear, forming a border which occupies one-third the width*

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### EXPLANATION OF PLATES.

Plate LXV.

Sphagnum fimbriatum.

*a*.—Fertile plant.
1.—Part of stem with a branch fascicle.
2.—Male inflorescence. 2 *b*.—Bract from same.
3.—Fruit and perichætium. 4.—Upper bract from same.
5.—Stem leaf. 5 *a a*.—Areolation of part of apex of same.
6.—Leaf from middle of a divergent branch. 6 *p*.—Point of same. 6 *c*.—Cells from middle × 200. 6 *x*.—Transverse section.
7.—Intermediate leaves from base of a divergent branch.
8.—Leaf from a pendent branch.
9 *x*.—Part of section of stem. 9 *c*.—Outer cells of bark.
10.—Part of a branch denuded of leaves.

Plate LXVI.

Sphagnum strictum.

*a*.—Female plant. *o*.—Male plant.
1.—Part of stem with a branch fascicle.
2 *b*.—Bract from male inflorescence with an antheridium.
3.—Fruit and perichætium.
4.—Bract from same. 4 *a*.—Apex of same expanded.
5.—Stem leaves. 5 *a a*.—Areolation of apex of same.
6.—Leaves from a divergent branch. 6 *p*.—Point of same. 6 *c*.—Cells from middle × 200. 6 *x*.—Transverse section.
7.—Basal intermediate leaf.
9 *x*.—Part of section of stem.
10.—Part of a branch denuded of leaves.

*of base, but rapidly narrows and disappears half-way up the margin.*

*Lower ramuline leaves broadly ovato-lanceolate, upper elongate lanceolate, acute, with a narrow border;* hyaline cells with annular and spiral fibres, and a row of large pores on each side; chlorophyll cells compressed, enclosed by the hyaline, but nearest to the upper surface of leaf.

*Male amentula elongated, fusiform, yellowish-green,* the bracts ovate, acute. Capsules at first immersed in the large imbricated perichætium, afterwards becoming moderately exserted; *bracts below obovate-oblong, above very broad, convolute, cucullate when young, obtuse or emarginate or with a small central apiculus,* rather laxly areolate, without fibres or pores. Spores ferruginous.

Hab.—Bogs and marshy hollows, not uncommon; frequent in Lancashire and Yorkshire. Fr. June and July. Brought from the Antarctic regions by Dr. Hooker, and found throughout Europe and N. America.

This slender and elegant species is readily distinguished from *Sph. acutifolium* by its pale green colour, and large rounded fringed stem-leaves; it is more closely allied to *Sph. strictum*, but that species is dioicous, and much more robust, and by comparison of the stem leaves of the two plants, we at once see that they differ in form, and that in the latter the fringed portion is restricted to the straight truncate apex, while in *Sph. fimbriatum* it extends part of the way down the lateral margin. The specimens given under No. 718 in Rabenhorst's Bryotheca as *Sph. fimbriatum*, belong to *Sph. strictum.*

### 13. Sphagnum strictum Lindberg, MSS.

Öfvers. Vet. Ak. Förhandl. XIX, p. 138 (1862).

PLATE LXVI.

Syn.—*Sph. Girgensohnii* Russow Torf. p. 46 (1865). Milde Bry. Siles. p. 387 (1869).

*Dioicous, very like Sph. fimbriatum, but more robust, yellowish-green or pale brownish, in loose tufts.* Stem straight pale, 6–10 inches high, with 3–4 layers of porose cortical cells. *Cauline leaves erect, appressed to stem, ligulate-spathulate, truncate and laciniate-fimbriate at apex, but not below the rounded apical angles;* hyaline cells of upper part rhombic, of middle base rhomboidal, free from fibres and pores, *lateral at base very narrow, and with the chlorophyll cells forming a very broad border extending up to apex.* Ramuli 3–4, of which 2–3 are spreading, flagelliform, the others deflexed, filiform, appressed to stem, retort cells elongated, perforated, scarcely recurved. *Ramuline leaves erecto-patent, ovato-lanceolate and lanceolate,* sometimes recurved at apex; hyaline cells

with annular and spiral fibres and numerous large pores; *chlorophyll cells trigonous, compressed, nearest the upper surface of leaf.*

*Male amentula numerous, elongated, clavate, thickish, ochraceous or brown,* the antheridia confined to the terminal part; bracts broadly ovate, acuminate.

Fruit in the capitulum or upper part of stem, *bracts pale green, the lower ovato-acuminate, upper obovate-oblong, convolute, obtusely pointed, rather densely areolate, without fibres or pores.*

Var. *β, squarrosulum* Russow.

Plants very small, branch leaves recurved at apex.

Hab.—Shallow bogs on subalpine heaths unmixed with other species; frequent in Central and Northern Europe. In this country it has been found on Ben Ledi, by Dr. Stirton; at Killin, Ben Lawers, Stronach rocks in Glen Lyon, and Banchory, by the late Mr. Hunt; and at Dent, Skegglesmere, &c., Westmoreland, by Mr. Barnes.

The Irish specimens named *Sph. Girgensohnii* in Dr. Moore's Synopsis, belong to *Sph. acutifolium.* This species stands intermediate between *Sph. acutifolium* and *Sph. fimbriatum,* and has no doubt been frequently mistaken for both, but by the characters and figures now given it ought to be readily distinguished. The fruit is very rare, and for both the specimens figured I am indebted to the kindness of my friend Professor Lindberg, who collected them near Helsingfors.

## NEW BOOKS, WITH SHORT NOTICES.

Evenings at the Microscope; or, Researches among the Minuter Organs and Forms of Animal Life. By Philip Henry Gosse, F.R.S. A new edition. London: Society for Promoting Christian Knowledge. 1874.—There are very few microscopists who will not be pleased to learn that a new edition of Mr. Gosse's excellent treatise has been produced. Indeed, it is so long since the work first appeared, that to many of our observers with the microscope it will be entirely a new book. But it must be confessed that to those who are already acquainted with the volume as it was at first issued, the new edition will not convey much more information than the old one. We may as well state this at the outset, that we think Mr. Gosse has failed in his editorial labours, for he most assuredly has not brought the book up to even ten years before the date of publication. This fact and the insufficient presence of engravings are the only faults we have to complain of; all else is as excellent as anyone who is familiar with Mr. Gosse's many able treatises on Natural History can readily imagine. The subjects which the author has undertaken to describe, and which are dealt with as alone Mr. Gosse is able, are of course familiar to the Fellows of the Royal Microscopical Society; at least all save one or two of the more rare and singular forms, of which Mr. Gosse has been the first and almost the sole observer. Such subjects as, for example, human hair, and the hairs of the bat, the mouse, the cat, and sheep; scales of the commoner fishes, blood disks, circulation in the frog's foot, scales from gnat's wing, bristle-tail, *Polyommatus*, *Pieris*, &c.; the spiracles of flies, the mouth of a beetle; the zœa and other stages of the crab; the anchor-plates of synapta; the polyps of cows' paps, such are familiar enough to the general microscopic observer. Some others, however, are by no means so common; such, for example, is the *Lar Sabellarum*, of which Mr. Gosse was for years the sole observer, and which has even now been only seen by him and by the Rev. T. Hincks, F.R.S., who published a paper upon this very peculiar animal in the 'Annals of Natural History,' November, 1872. Of this singular form a very good woodcut is given, and a capital description accompanies it. This animal was observed upon the anterior part of the Sabella's tube; as Mr. Gosse says: "About twenty bodies, having a most ludicrously close resemblance to the human figure, and as closely imitating certain human motions, are seen standing erect around the mouth of the tube, now that the Sabella has retired into the interior, and are incessantly bowing and tossing about their arms in the most energetic manner. . . . . A slender creeping thread irregularly crossing and anastomosing, so as to form a loose network of about three meshes in width, surrounds the margin of the Sabella's tube, adhering firmly to its exterior surface, in the chitinous substance of which it seems imbedded. Here and there free buds are given off, especially from the lower edge; while from the upper threads spring the strange forms that have attracted our notice. These are

spindle-shaped bodies about the $\frac{1}{40}$ of an inch in height, whose lower extremities are of no greater thickness than the thread from which they spring; with a head-like lobe at the summit, separated from the body by a constriction, immediately below which two lengthened arms project in a direction towards the axis of the tube." Such is the description of the animals themselves, but the account of their movements is still more wonderful. "The head-lobe of each one moves to and fro freely on the neck, the body sways from side to side, but still more vigorously backward and forward, frequently bending into an arch in either direction; while the long arms are widely expanded, tossed wildly upward and then waved downward, as if to mimic the actions of the most tumultuous human passions." The author then proceeds to describe it minutely, and he gives the various ideas which he formed of its nature and the class to which it belonged, eventually coming to the conclusion that it is a hydroid polyp of the family Corynidæ. Further as to its specific name, Mr. Gosse says: "When I see them surrounding the mansion of the *Sabella*, gazing as it were after him as he retreats into his castle, flinging their wild arms over its entrance and keeping watch with untiring vigilance until he reappears, it seems to require no very vivid fancy to imagine them so many guardian demons; and the Lares of the old Roman mythology occurring to memory, I described the form under the scientific appellation of *Lar Sabellarum*."

The author's remarks on the subject of the *cnidæ* of sea-anemones are likewise of great interest, for he evidently speaks from considerable experience on the question. His chapter on these peculiar organs deserves careful perusal; meanwhile we may just quote his concluding observations as to the possible function, and its mode of performance, of these singular organs. He says, "admitting the existence of a venomous fluid, it is difficult to imagine where it is lodged and how it is injected. The first thought that recurs to one's mind is, that it is the organic fluid which we have seen to fill the *cnida*, and to be forced through the everting tubular *ecthoreum*. But if so, it cannot be ejected through the extremity of the *ecthoreum*, because if this were an *open* tube, I do not see how the contraction of the fluid in the *cnida* could force it to evolve; the fluid would escape through the still inverted tube. It is just possible that the barbs may be tubes open at the tips, and that the poison fluid may be ejected through these. But I rather incline to the hypothesis that the cavity of the *ecthoreum, in its primal inverted condition, while it yet remains coiled up in the cnida*, is occupied with the potent fluid in question, and that it is poured out gradually within the tissues of the victim, as the evolving tip of the wire penetrates further and further into the wound."

We trust we have quoted our author sufficiently to show that while his style is simple, forcible, and clear, it is moreover purely scientific, and we hope we have shown that his book is full of interesting and instructive matter. We heartily wish it the success it so well deserves.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Placental Circulation* has been again investigated; this time by M. Delové, who read a paper before the French Biological Society. Whatever value may be attached to his researches, the following facts are of interest:—(1) An injection made by the circular sinus penetrates the whole of the placenta, and the same thing takes place if an injection be made by the umbilical vessels. (2) The placenta of the still-born infant, of which the blood has lost the colouring matter, shows recent clots in its interior. (3) All the collected sections of the placenta show villosities in contact with blood corpuscles. (4) The presence of the vascular epithelium in the placental sinuses is a further proof that blood passes through them.

*A Nervous System in the Actinia.*—Professor M. Duncan, F.R.S., has been making some very valuable researches on this point, and has published the results, with a couple of excellent plates, in a late number of the 'Proceedings of the Royal Society.' We hope to lay both the author's investigations and his plates before our readers, in an early number of this Journal.

*The Ovarian Egg and Early Development of Loligo.*—Under the title of "Contributions to the Developmental History of the Mollusca," Mr. E. Ray Lankester, M.A., has read a very valuable essay before the Royal Society, which will be published fully, with illustrations, in the 'Transactions.' The following is a short account of the chief points of interest in the section devoted to *Loligo*:—(1) The explanation of the basketwork structure of the surface of the ovarian egg by the plication of the inner egg-capsule. (2) The increase of the yelk by the inception of cells proliferated from the inner egg-capsule. (3) The homogeneous condition of the egg at fertilization. (4) The limitation of yelk-cleavage to the cleavage-patch. (5) The occurrence of independently-formed corpuscles (the autoplasts) which take part in the formation of the blastoderm. (6) The primitive eye-chamber, formed by the rising up of an oval wall and its growing together so as to form a roof to the chamber. (7) The origin of the otocysts by invagination. (8) The rhythmic contractility of a part of the wall of the yelk-sac. (9) The disappearance of the primitive mouth, and the development of a secondary mouth. (10) The development of a pair of large nerve-ganglia by invagination of the epiblast immediately below the primitive eye-chambers.

*How the Nerves end in the Cornea.*—Dr. Henry gives an account in the 'Medical Record' of a recent paper by Signor Durante, who has made observations on the subject at Rome. This observer states, that by soaking the cornea of batrachians, rabbits, and dogs, in solution of chloride of gold, and keeping them at a temperature of 88° Fahr., he was, at the end of four days, enabled to see a very distinct network of nerves. When a combination of nitrate of silver and chloride of gold is used, the intercellular substance is sometimes coloured; not the

fibre, which appears like a vein or capillary charged with nitrate of silver. In frogs, the nervous trunks, from six to fifteen in number, pass into the cornea at the level of the internal elastic coat, lose their medullary sheath, and subdivide into numerous fibres, which become interlaced, and form a plexus in the interior layers of the cornea. This plexus gives origin to many other fibres, which spread out in the anterior layers of the cornea, and give off branches at a right angle. These branches in their turn subdivide in the same way, forming a network of fibres through the whole thickness of the cornea. The axis cylinders become separated from the primitive fibres, and pass in a straight line to the cylindrical epithelium, among the layers of which they follow a winding course as far as the last layer but one, where they form an irregular network, which is best seen in sections made parallel to the anterior surface of the cornea. The arrangement of the nerves in the cornea of the rabbit differs from that in the frog, both in direction and in mode of distribution. The nerves, sixteen to twenty in number, penetrate the cornea towards the outer part, and form a plexus. The fibrils which penetrate the epithelium proceed from the fibres which pass obliquely outwards from the plexus. Beneath the epithelium the fibres are divided into tufts and fibrils, which penetrate the epithelium, some directly, others after a short passage, and form in the penultimate layer a very fine fibrillar network, with narrow meshes. Some of the fibrils form loops in the deep epithelial layers. In the dog, the nerve-trunks entering the cornea are smaller but more numerous than in the rabbit, and are distributed nearly in the same manner. Some of the fibres pass directly to the under surface of the epithelium, where they divide into numerous primitive fibrils, which run parallel to the anterior surface of the cornea. From these, other finer fibres are given off, which, having reached the last layer but one of the epithelium, form the network which has been already described. Sig. Durante did not find nerve-cells or corpuscles among the fibres, nor any termination of the nerves in cells.

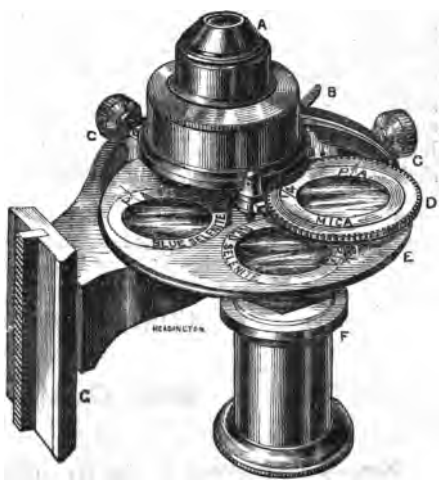
*The Intracellular Development of Blood Corpuscles in Mammals.*—At the meeting of the Royal Society, March 19th, Mr. E. A. Schäfer communicated a very valuable paper on this subject. He says that if the subcutaneous connective tissue of the new-born white rat is examined under the microscope in an indifferent fluid, it is found to consist chiefly of an almost homogeneous hyaline ground-substance, which is traversed by a few wavy fibres, and has a considerable number of exceedingly delicate, more or less flattened cells scattered throughout the tissue. The cells here spoken of are of course the connective-tissue corpuscles. They are not much branched as a rule (at any rate their branches do not extend far from the body of the corpuscle), and they are mainly distinguished by the extraordinary amount of vacuolation which they exhibit—by which is meant the formation within the protoplasm of minute clear spherules, less refractive than that substance, and probably, therefore, spaces in it containing a watery fluid. The nuclei, of which there is generally not more than one in each cell, are frequently obscured by the vacuoles, but, when visible, are seen to be round or oval in shape and beautifully clear and homogeneous;

they commonly contain either one or two nucleoli. It is from these cells that the blood-vessels of the tissue are formed, and within them, red, and perhaps also, white blood corpuscles become developed. Of the vacuolated cells above described some possess a distinct reddish tinge, either pretty evenly diffused over the whole corpuscle, or in one or more patches, not distinctly circumscribed, but fading off into the surrounding protoplasm. Others contain either one, two, or a greater number of reddish globules, consisting apparently of hæmoglobin. These vary in size, from minute specks to spherules as large as, or even larger than, the red corpuscles of the adult: in cells which are apparently least developed it is common to find them of various sizes in the same cell; whereas cells which are further advanced in development are not uncommonly crowded with hæmoglobin globules, tolerably equal in point of size, and differing from the adult corpuscle only in shape. It is important to remark that there is, at no time, an indication of any structure within the globules resembling a nucleus: the nucleus of the cell also appears, up to this point at least, to undergo no change. In fact, the formation of the hæmoglobin globules reminds one rather of a deposit within the cell-substance such as occurs in developing fat-cells, the difference being that in the latter case the deposited globules eventually run together into one drop, whereas in the former they remain distinct as they increase in size and eventually take on the flattened form. Before, however, this change occurs in the hæmoglobin globules, the cells containing them become lengthened, and are soon found each to contain a cavity, within which the globules now lie. This cavity is probably formed by a coalescence of the vacuoles of the cell, or, what amounts to the same thing, by the enlargement of one vacuole and the absorption of the rest into it. The cell now comes to resemble a segment of a capillary, but with pointed and closed extremities; it is of an elongated fusiform shape, and consists of a hyaline protoplasmic wall (in which the nucleus is imbedded) enclosing blood corpuscles in a fluid—blood, in fact. Two or more such cells may become united at their ends, a communication being established between their cavities; indeed, by aid of branches sent out from the sides a number of cells may unite to form a complete plexus of capillary vessels containing blood, and situate at a considerable distance in the tissue from any vessels in which blood is circulating. Eventually, however, these last become united with the newly-developed capillaries, and the blood contained in the latter thus gets into the general circulation. With regard to the mode of junction of the capillary-forming cells with one another, and with processes from pre-existing capillaries, it has seemed to the author to occur most commonly, not by a growing together of their extreme points, as commonly described, but rather by an overlapping and coaptation of their fusiform ends, which, at first solid, become subsequently hollowed by an extension into them of the cavity of the cell or capillary, the partition between the two being finally dissolved.

## NOTES AND MEMORANDA.

**Death of Herr Max Schultze.**—We very much regret to say that Herr Max Schultze, the distinguished German anatomist and histologist, is dead. He was the editor of the well-known 'Archiv für Microscopiche Anatomie,' devoted largely to the anatomy of the tissues and to the infusoria. He wrote also on the embryology and anatomy of the worms, of echinoderms and hydroid medusæ, and on the foraminifera; and he was noted among human anatomists more especially for his several splendid essays, some of which have been translated into English, upon the Histology of the Retina. He was born in 1825, and died, in the prime of life, at Bonn, having just had completed for his use, as is said, the amplest and most elegantly constructed laboratory in Europe.

**A New Form of Achromatic Condenser.**—This form is constructed to supply the place of a compound sub-stage, together with all ordinary sub-stage apparatus. The optical combination (see figure) A is so contrived that it can be used as a very effective spot lens from the 3-inch objective up to the $\frac{4}{10}$ths, and having an angle of aperture of 140° enables it to be worked with a $\frac{1}{25}$-inch objective, giving a well-defined image of the object under examination without blur or mist. B is a small lever by which the contracting diaphragm is opened and shut; C, C, two small milled heads by which means the optical combination A is centred to the axis of the objective. The revolving diaphragm E has four apertures for the purpose of receiving central stops, oblique light disks, and selenite films, as shown in the engraving. D is a frame carrying two revolving cells, into one of which a mica film is placed which can be revolved with ease over either of the selenites below, whereby all the changes of tint and colour can be obtained that are required in manipulating with polarized light. The darts and P, A shown on the engraving indicate the position of the positive axis of the mica and selenite films, by which means former results can always be recorded. The other cell is of service for receiving oblique light stops, &c. Either of the two revolving cells can be thrown into the centre of the condenser and stopped in their proper position by means of a spring catch: when thus



arranged the mica film, &c., is revolved in its place by turning the cell D, as both cells are geared together with fine racked teeth. F is a polarising prism which is mounted on an eccentric arm, and can be brought central with the axis of the condenser when required in use, or thrown out as shown in woodcut when not wanted. G is a rack dovetail slide fitting into microscope for the purpose of focussing the condenser on the object. The advantage of this condenser over all other contrivances of this description is that the polarizing prism, selenite films, dark ground and oblique light stops, are brought close under the optical combination A. The instrument has been devised by Mr. James Swift.

**Mr. Tolles' $\frac{1}{75}$th Objective.**—The 'Boston Journal of Chemistry' announces that Mr. Tolles has alone made an object-glass of this power, but that Journal is evidently unfamiliar with the fact that Messrs. Powell and Lealand have long since achieved a similar result. The paper in question says, "Boston stands pre-eminent in the production of exquisite and wonderful optical instruments. Mr. Tolles has just achieved the great result of producing a $\frac{1}{75}$ objective for microscopic uses, a glass of such difficult construction that we believe no optician has ever attempted it before. The power of this objective is such that a single white blood corpuscle covers the entire field of vision. Mr. Tolles has produced two of the finest $\frac{1}{50}$ objectives ever constructed, one of which is in this city, the other in the hands of a Western gentleman. The angular aperture of one is 120°; that of the other and the last constructed, is 165°. The objectives are of great excellence, *and, in the opinion of competent microscopists, far surpass in defining power and clearness of field those of European make.*" ! !

## CORRESPONDENCE.

### Zeiss's Object-glasses.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—I have received from W. G. Lettsom, Esq., some particulars of the objectives made by Zeiss, of Jena, and I have had, by the kindness of the same gentleman, an opportunity of trying one of his small-angled and low-priced $\frac{1}{8}$ths. This glass is the D of his catalogue, and the angle of aperture is stated at 60°. I have found this glass so useful, on account of its working distance, penetration, and excellent action with dark-ground illumination and eye-pieces A, B, and C, of the Ross series, that I am induced to call the attention of English microscopists and opticians to the advisability of not confining their operations to glasses of large angle, as is too usually the case. The small-angled $\frac{1}{8}$th would not compare for a moment with a large-angled English $\frac{1}{4}$th or $\frac{1}{5}$th in resolving troublesome diatoms; but for many

objects it has a decided advantage over the large-angled patterns, even when their corrections are more perfect than its own.

Zeiss makes three immersion glasses, Nos. 1, 2, 3, with focal lengths in millimètres 3, 1·7, and 1 respectively; one millimètre being 0·039 of an English inch. He states that the highest of these "may be used very well with a covering glass of above a fifth of a millimètre in thickness." The power of this glass is stated to range from 860 to 2400, according to the eye-piece used. The angles of the three immersion objectives are stated at 180°; but the following remark is made in the catalogue, and may be taken as a contribution to some of the discussions now going on.

"The indication contained in the Table touching the angle of aperture of the immersion systems does not treat the matter exhaustively, inasmuch as in the case before us the amount of the angle cannot be at all stated with accuracy in the *ordinary* way, that is to say, for air as the external medium. The real angle of aperture of the above immersion systems lies between 104° and 108° for *water*, whereas an angle of only 97° for water would give an angle of 180° in air.

"That when compared with this great angle of aperture in the immersion systems, the dry systems, even in the highest powers, should have an angle of only 105°, that is to say, a materially less angle than the high systems of numerous other makers, especially the English opticians, is to be thus accounted for, namely, in consequence of theory and practice agreeing in assigning 105° as the limit that may not be exceeded in *dry* systems without either rendering the entire correction of the spherical aberration impossible, or reducing the distance of the object so considerably that the systems become in consequence extremely troublesome to use."

The catalogue states that the whole of the object-glasses "are constructed in conformity with the theoretical calculations of Professor Abbe, of Jena." Are these known to our opticians? and do they differ from the formulæ they employ, either in mathematical accuracy or in facilities for cheap construction? This may be a point worth looking to.

Yours, &c.,

HENRY J. SLACK.

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## MR. WENHAM'S CRITICISM OF DR. WOODWARD'S FIGURE.

*To the Editor of the 'Monthly Microscopical Journal.'*

GEORGETOWN, D.C., *May* 6, 1874.

SIR,—I think Mr. Wenham's criticism of Dr. Woodward's figure* likely to mislead, for the reason that a well-known fact is advanced as if inconsistent with the relative positions of the points F, F', &c., in that figure. It is true that when a glass cover is placed over an object in focus, the objective has to be drawn away and the distance slightly increased. But it is also true that the object is at the same time apparently brought towards the objective, and becomes an *apparent*

* 'M. M. J.,' No. lxiv., p. 170.

*radiant* between the objective and the true radiant. No adjustment of the objective can change the relative positions of these two radiants, which are correctly given in the figure. Otherwise the criticism seems to me irrelevant, as the only use of the figure was to prove that a ray making a greater angle than 41° with the axis could pass *into* the objective, which seems to be no longer denied.

The optical principle upon which was based a theoretical objection to Mr. Tolles' objective of 87° immersion angle, is that of *total reflexion*. That objection being abandoned, the discussion turns upon the course of the rays after entering the objective. It will be found that there is nothing mythical about the back lenses of the instrument in question, but that they are capable of bringing to a sharp focus rays emanating from an object in balsam at an angle of 87°.

One maker having made a myth of the "limit," it is probable that the rest will soon follow. But Mr. Tolles richly deserves high praise from all who use microscopes, and all who make them, for perseverance in the mechanical expression of his correct perception of the case, in opposition to high theoretical authority.

Respectfully, &c.,

R. KEITH.

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## THE APERTURE QUESTION.

*To the Editor of the 'Monthly Microscopical Journal.'*

LONDON, *May* 8, 1874.

SIR,—Having heard complaints from several quarters that the aperture discussion has become tedious so far as it relates to the point, whether a few degrees beyond the theoretical limit has been obtained or not in an immersion lens upon an object in balsam, the time has come when the question may rest upon its own merits, as the late personalities must appear discreditable by neutral readers who care for the science only. It happens that Mr. Tolles, who is stated to have accomplished the feat, is an optician obtaining his income from the construction of object-glasses, consequently trade feeling has crept in, the acrimony of which might have been avoided had this not been so. The whole evidence against theory and myself has rested on what Mr. Tolles is said to have done. On this the question may be at an end; for the first object-glass sent for trial did not show any such result, neither does the last, now in the possession of Mr. Crisp, though specially intended to do so, but proves quite the contrary, even from the evidence of dimensions given by the front lens itself. I submit that I was not bound to be reticent because Mr. Tolles as a maker chooses to engage in controversy. The coming of the first glass was announced by a kind of challenge that it had qualities which "no English glass would be found to possess," and curiosity was attracted. I had not the slightest wish to damage the occupation of Mr. Tolles or his agent, and believe that no injury has been done, for Mr. Tolles now probably considers that form obsolete, and not to be repeated. All those that made comparisons of the former glass

against their own, will, I think, agree with me that the last is superior. I may mention that this has a "doublet front" consisting of two single plano-convex lenses, as some discussion of the merits of such a form both in theory and practice may be appropriate, and result in some useful knowledge.

I must remind my readers that the first definite information on the subject of the modern object-glass was published by myself in the early numbers of this Journal, in what had before been rather a matter of secresy. And on the information derived from my writings, I scarcely expected that I was to be blown up with my own petard. But so long as the fabric is begun from the wrong end, it must finally fall. We all know how the achromatic telescope has benefited by the labours of Herschel, Barlow, and Airy; their formulæ were worked from a true basis of parallel rays, and the aberrations considered relative to their path to a final focal point, thus beginning from a definite and known position. So it should be with the microscope object-glass, for in order to treat it diagramatically and mathematically, the course of the rays must be commenced from the long conjugate focus at the back, near the eye-piece, which can be taken as a correct and absolute position, and in a theoretically accurate combination the anterior focus must then follow with certainty. To attempt to arrange a construction from an assumed focal point in front, in hopes of defining the aberrations, is about as senseless as trying to build up a pyramid from its apex. It is no wonder, then, that those attempting this get no further than the front lens, and there abandon the argument. So long as this course is adopted, the debate, thus starting from a baseless foundation, can never arrive at a conclusion, and it is therefore useless to continue it.

I anticipate that this peremptory dismissal may bring its comment in the late style; I will, however, add that I shall respect a diagram illustrative of the immersion principle, in which the back combinations serve the twofold purpose, and where in both cases the rays must follow the same course and direction up to the front lens—for the immersion principle lays beyond this. I will take into consideration any correctly-drawn demonstration that will show a result.

I am yours very truly,

F. H. Wenham.

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## Rusticus, jun., on Mr. Brakey's Reply.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—Believing the Rev. Mr. Brakey to be willing—nay, anxious to try his dialectic on any question involving an "Optical Curiosity of Literature," I ventured to request his consideration of the two passages which I placed in juxtaposition: the one written by himself, the other by Mr. Wenham. Instead of reconciling the statements—his own and Mr. Wenham's—which he had previously said he knew "must coincide," he lays down this paradoxical theorem: "In the two pencils [*i. e.* with immersion—and with dry lens] light is lost unequally, one

losing by common reflexion, while the other loses more by total reflexion; but the angles themselves remain equal."

Fixing our attention on these two pencils, let us see where this theorem leads us. He has admitted that "more of the pencil of light is lost in the air lens than in the other from *total* reflexion," which disposes of pencil No. 2—the dry lens pencil. And, now, applying his theorem we learn that *more of the pencil of light is lost in the immersion lens from common reflexion!* which disposes of pencil No. 1—the immersion lens pencil—in a passing strange manner, not admitting of proof by diagram.

Moreover, in the 'M. M. J.,' No. xxii., p. 238, Mr. Brakey says "the immersion lens would lose somewhat less light than the other by ordinary reflexions."

I do not mean to suggest that Mr. Brakey cannot explain his paradoxes; but till he does so in an intelligible form, I shall, with sorrow, be obliged to hold to my "not flattering" opinion of his writings.

Your obedient servant,

——— RUSTICUS, jun.

## MR. TOLLES AND DR. ROYSTON-PIGOTT.

*To the Editor of the 'Monthly Microscopical Journal.'*

ILKLEY, NEAR LEEDS, *May* 14, 1874.

DEAR SIR,—I have received a letter enclosed by Mr. Stodder from Mr. Tolles, in which he expresses his opinion "that most readers will confound angle aperture with field aperture."

"Why not write Dr. Pigott, calling his attention to the difference between reduced aperture and reduced field?"

He goes on to say, quoting my expression—"Bad telescopes cured of residuary aberration by contracting the aperture" never define well *anywhere* with the *full aperture* (this everybody knows well enough). "But the ¼-inch objective defines just as well with the *full aperture* as under any degree of contraction: only the maximum (the best) effect is gained through but a limited extent of field."

"Dr. Pigott must know about this distinct difference between angle aperture and field aperture, while *most readers* will confound the two as he does in expression. If you will call the Doctor's attention to the matter, no doubt he will make correction."

To these remarks I hasten to say that I am sorry Mr. Tolles should think it necessary to write about this matter, as I suppose very few persons whose opinions are worth anything would make so egregious a blunder as to imagine using an eye-piece with a very small field of view contracted the angular aperture of the objective. The point of my remark in the foot-note, page 175 of last number of the Journal, is substantiated by Mr. Tolles' own admission, namely, that, as he says, "only the maximum effect is gained through but a limited extent of field."

Indeed, I stated in effect "that in a limited field of view the definition of Tolles' ⅛th is superb: beyond the central area the definition

is very indistinct." This seems to indicate that the very oblique pencils are not as free from aberration as the central. But I agree with Mr. Tolles that contracting the angular aperture would not improve the definition of the peripheral or extreme field of view.

The eye-piece I employed was the ordinary Huygenian, furnished with the *usual stop* employed to limit the field of view, and which was by no means larger than effective with the English object-glasses.

I am yours faithfully,

G. W. Royston-Pigott.

## PROCEEDINGS OF SOCIETIES.

### Royal Microscopical Society.

King's College, *May* 6, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read, and the thanks of the meeting were voted to the donors. One slide, a poison fang of British viper, presented to the cabinet of the Society by Mr. Elliott, was exhibited under a microscope in the room.

The Secretary read a paper by Dr. Anthony "On the Suctorial Organs of the Blow-fly," the subject being illustrated by several beautifully-executed drawings. The paper will be found printed at p. 242.

The thanks of the meeting were unanimously voted to the author.

Mr. B. T. Lowne said there were one or two points to which he should like to draw attention. The paper itself was a very interesting one, but it contained one or two applications of terms which he thought were liable to lead to very serious misconceptions. The first of these was the term proboscis. Now this term had always been applied to the whole structure of the fly's tongue, but Dr. Anthony appeared to apply it only to the false tracheal tubes which lay upon the surface of the tongue. All naturalists were, however, agreed in calling the whole organ the proboscis, from its supposed resemblance to the proboscis of an elephant. Next, with regard to the so-called suckers, as Dr. Anthony described them. The term had always been applied to the whole surface of the disk. Mr. Lowne here drew upon the black-board large diagrams of the proboscis and the suctorial disk, and explained by reference to them the structure and action of the suctorial lobes. What he had called the false tracheal tubes Dr. Anthony had called the proboscis; they were, however, strictly speaking, not tubes at all, but were merely channels in the skin which were kept open by means of the chitinous rings which had already been described. The wavy lines seen between the false tracheal rays were really a number of little depressions containing small papillæ, as shown in his work on the subject.* These papillæ he had little doubt were tacti,

* 'The Anatomy and Physiology of the Blow-fly,' pl. 4, fig. 3.

or tasting organs. If the tongue of the living fly were examined it would be found constantly moist, and it was believed that the moistening secretion passed out either through this wavy line or through the tissue itself. Dr. Anthony had spoken of the muscles as being the means of closing the false tracheal tubes: now this appeared to him to be a mistake; he could only suppose that the muscles were used to open the tubes, and that in their normal condition they remained closed. He must totally dissent from Dr. Anthony's notion as to the pumping action of the interior of these tubes; he had examined hundreds of them, and had never seen anything like it. He had given some blood to a fly and then watched the creature feed upon it, and had seen the blood corpuscles flowing up these tubes with great regularity without any pumping or pulsating action whatever. He also dissented from this idea because in the upper part of the proboscis there was a real pumping apparatus. In action all the little canals served as feeders to the great central canal, and when this was full of fluid the valve action drew it up and forced it into the alimentary canal; he had carefully examined the part which he had just described and figured on the board, and had no doubt whatever that it was a real pumping organ. The drawings which Dr. Anthony *had* sent with his paper were very, very nice, and were, he believed, quite correct, only he had not seen those ear-like processes which were shown on one of them; if they had been in existence in any of the very numerous specimens he had examined he felt sure he must have seen them, and as he had not observed them in any case he was disposed to think they must be due to something in the mode of dissection or preparation. Though he did not think that the tubes were suckers at all, he thought they might be really filters or strainers to prevent particles which were too large from running up the central tube and choking the pump.

The President said that having listened to the paper, and having seen the very beautiful drawings by which it was accompanied, he was inclined to put much more faith in them than Mr. Lowne did. So far as his own observations went, there was one character common to all suctorial organs, whether they were of this kind or such as the foot of the fly—they all consisted of a margin of soft tissue and an internal part capable of being raised by some muscular action. He had long been perfectly familiar with the chitinous rings of the fly's tongue, and had often asked himself the question how they acted, and he had never been able to see in what way they were able to take up fluids in the way they did. As regarded the whole tongue, he was not aware that it had such a margin as would enable it to act, as a whole, as a sucking organ; but if each of those spoon-shaped bodies shown by Dr. Anthony were placed as drawn, then their action became perfectly intelligible, and it was perfectly clear that they would act in the manner stated. It was quite clear that if there were muscles on the opposite sides of these chitinous rings, and if the normal position of the rings was to be closed, that they would, in the act of opening, at once give rise to a suctorial action; and if the relaxation of the muscles closed the tube, it was quite likely that by the collapse of the channel of communication between these spoon-shaped suckers the

fluid would be urged forward into the main tube. The explanation seemed to be clear, and the purpose admirably performed if it were as described, and it seemed to him to be a perfectly feasible method of performing a process which he had never before been able to see.

Mr. Lowne said he should like to make one or two observations upon the President's remarks as to the action of these so-called suckers. He had yet to learn that a force-pump could be made to act without a valve; he had yet to learn that a tube open at both ends could be in any way exhausted. It was all very well to talk in this way and say it *might* be—anything *might* be, but what he was talking about was what actually *was*, and he had yet to learn that such organs existed. He thought, certainly, that the notion of the suctorial action of the fly's foot and of the foot of Dytiscus was exploded long ago. He had years ago brought into that room a living Dytiscus placed under the exhausted receiver of an air-pump, and had shown that even there it was able to support a weight of several ounces. He thought it had long ago been shown that this was done by means of a glutinous fluid which exuded from the feet of flies and beetles. Of course it *might* be, but he was only speaking to facts.

Mr. Charles Stewart said that as the question of correct terms had been raised, although it might look like hypercriticism, he should like to ask Mr. Lowne whether he was justified in calling the little watch-tower looking object which he had drawn a papilla. Was it not rather an aborted hair?

Mr. Lowne said Mr. Stewart could call it what he liked; there was not much importance in the term, but he thought the object was totally unlike a hair.

Mr. H. J. Slack read a paper "On certain Silica Films artificially produced," illustrating the subject by drawings from the pencil of Dr. Anthony, and by specimens exhibited under a microscope in the room. The paper will be found on p. 237.

The President said that on looking at the drawings he was struck with the remarkable resemblance of some of them to the anchors of the Synapta.

Mr. Wenham had been presented with a slide by Mr. Slack, upon which he found a curious spinous form, which he saw was figured by Dr. Anthony. He found on examination that the particles had arranged themselves in this spinous form, and he should be very glad to be informed as to the cause of this arrangement.

Mr. Slack said that the films were very delicate, and appeared to be formed of innumerable spherules; and it was when another layer was deposited upon them that these apparently organic forms, like bacteria and fungi, were produced. They were very easily detached. He was inclined to think that some crystalline forms were due to a silicate of some alkali. Directly the silica molecules were induced to depart from their usual way of violently rushing together, they would readily adopt organic patterns upon the slightest impulse (a diagram in illustration was here drawn upon the board).

The President thought it perhaps possible that the apparent amplification by lower powers to which Mr. Slack had referred might be due to irradiation, just as a very fine platinum wire appeared to

increase in size as it was made red hot. It just struck him that this appearance might be the result of the greater amount of light passing through these objects, and producing irradiation with lower powers.

Mr. W. T. Read said that he had been for some time much interested in silica, but he had always obtained it for purposes of experiment from a silicate of potash which he neutralized by various acids. If he took a solution of silicate of soda and added hydrochloric acid, then this rushing together took place; but if a weaker solution and a weaker acid were used, such as sulphurous or even carbonic acid, they would find that after a time, say in a day or two, the liquid, instead of resolving itself into a jelly, would become milky or cloudy in appearance, which he thought was the result of innumerable spherules being deposited, but yet remaining suspended in the fluid. This condition he regarded as intermediate between silica in solution and silica in a gelatinous form.

Mr. Slack said that he had never examined them exactly in that way; but he thought if a milkiness were obtained, there would be a chance of seeing the particles.

Mr. Read said that if a solution of silicate of soda was obtained, and it was neutralized with hydrochloric acid and then dialyzed, they would obtain silica simply in solution. Some of this solution had been obtained which had remained liquid for a great period of time, and he thought that this was due to its having been accurately pure; but it was easily disturbed, and a very little disturbance would cause it to gelatinize. If it were taken of a certain density, it would be found to assume a cloudy condition preliminary to the particles rushing together and forming a jelly. This milkiness was increased when the strength of the solution was reduced.

Mr. Slack thought there was a great tendency to coalesce without assuming a spherical form. He should much like to see it in the state described by Mr. Read.

Mr. Read said he should have great pleasure in forwarding a sample of it. He had a practical object in view in making his experiments; and he believed that silica was a substance which would presently become of great interest. He had already found out chemical properties of a very remarkable character; but he found that silica in solution was very apt to rush into the amorphous form.

A paper by Dr. Pigott, "On the Use of Black Shadow Markings and on a Black Shadow Illuminator," was (owing to the lateness of the hour) taken as read, the apparatus described being placed upon the table for the inspection of the meeting. The paper will be found printed at p. 246.

Mr. Wenham thought that it was an extension of the principle which had already been brought under their notice. He had himself used tin-foil above the objects, and the condensing apparatus under the slide.

Mr. Frank Crisp observed that Dr. Matthews brought out a plan for tilting the condenser in the same way, and had several of them made by Mr. Swift.

The gentleman elected on the 1st of April was Robert Horne, Esq., not Herbert Horn, as printed in the last number.

Donations to the Library and Cabinet since April 1st, 1874 :—

| | From |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| Journal of the Linnean Society. No. 75 | *Ditto.* |
| Memoirs of the Literary and Philosophical Society of Manchester. Vol. 4. 1871 | *Ditto.* |
| Proceedings of Sessions of ditto, from 1868 to 1870 | *Ditto.* |
| Verhandlungen der Kaiserlich-Königlichen-Zoologisch-botanischen Gesellschaft in Wien. 1873. | |
| Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania for 1872 | *Ditto.* |
| Flora of Middlesex. By Dr. Trimen and W. T. Thiselton Dyer. 1869. | |
| Preparation and Mounting Microscopic Objects. By Thomas Davis. 2nd edition. 1873. | |
| A Manual of Botanic Terms. By M. C. Cooke. 2nd edition. 1873. | |
| Our Reptiles. By M. C. Cooke. 1865. | |
| Bulletins de la Société Royal de Botanique de Belgique. 11 vols. 1862 to 1873 | *Ditto.* |
| A History of British Quadrupeds. By Thomas Bell, &c. 2nd edition. | |
| One Slide—The Poison Fang of the Viper *Pelias berus* | *Mr. A. C. Elliott.* |

Dr. G. Paddock Bate was elected a Fellow, and Mr. Henry G. Hanks, of San Francisco, a Corresponding Fellow of the Society.

WALTER W. REEVES,
*Assist.-Secretary.*

## MEDICAL MICROSCOPICAL SOCIETY.

At the eleventh ordinary meeting of this Society, held Friday, Feb. 20, at 8 P.M., Jabez Hogg, Esq., President, in the chair, the minutes of the previous meeting were read and confirmed; the names of three gentlemen for proposal were read, and three other gentlemen were elected members.

In the unavoidable absence of the Secretaries, the Treasurer, Mr. T. C. White, read a communication from Mr. Groves "On Cataloguing and Arranging Microscopic Specimens." After describing the difficulties generally experienced, he said that he had adopted a method at once simple and of universal application. For small collections he advocated a total absence of classification in the cabinet, though for large collections he considered it necessary. The catalogue he used, and which he would rely upon in all cases rather than the classification of specimens, was this:—He took an ordinary alphabeted notebook, and in that noted under the proper alphabetical heading every portion of every preparation. Thus, for a specimen of small intestine:—Under (I) was entered—Intestine, small, No. —; then under (G), Glands, Brunner's, No. —; Peyer's, No. —. Under (V), Villi, No. —; Villi, lacteals of, No. —; Villi, invol. muscle of, No. —, and so on. This method he found very handy for specimens required for demonstration purposes.

A vote of thanks having been passed, the President said he

thought the method proposed would supersede all others in use at the present time.

Mr. Needham endorsed these remarks, and said he had been in the habit of classifying his slides in physiological series, thus:—Respiratory, digestive, &c.; but this system had one great objection, which Mr. Groves' entirely obviated, *viz.* that one carefully-prepared slide might show a number of things which would entitle it to be placed in several series, but it could not be so placed, and consequently there was great and unnecessary multiplication of specimens, and, moreover, there was often some difficulty in finding any given preparation, which would not be the case with the system proposed.

Mr. Giles, Dr. Matthews, Dr. Donkin, and Mr. R. B. Miller also joined in the discussion.

Mr. Sidney Coupland then proceeded to make some remarks on some preparations which he exhibited of "Tuberculosis of the Choroid coat of the Eye," in a child æt. eight years. After describing the normal structures, and stating that he intended to confine himself wholly to the histological characters, he said that on removing the retina the tubercles were seen as translucent bodies, averaging $\frac{1}{30}$" in diameter, the centres of which were mostly opaque and white from degenerative changes. The chorio-capillaries could be traced partially over the tubercles. There was a marked deficiency of pigment and a notable increase in the number of the large pale spheroidal bodies. The tubercles were composed of nucleated cells, $\frac{1}{4500}$" to $\frac{1}{3000}$" in diameter, and with these were seen some larger and variously-shaped cells, having more than one nucleus, some of which were possibly derived from the normal pale spheroidal cells, though these were quite as numerous.

The tubercles appeared to arise from the middle layer of the choroid, and always around the blood-vessels. In the older tubercles, the central portions were made up of semi-fibrous and caseous material, the peripheral alone exhibiting the small cell growth. From this distribution it was evident that the growth was perivascular, and thus had probably arisen from a proliferation of the endothelia of the lymphatics, as in tubercle of the pia mater.

A vote of thanks having been accorded to Mr. Coupland, the President, Messrs. Cowell, Power, Atkinson, Needham, and Miller joined in the discussion.

In reply, Mr. Coupland said he had not examined the retina microscopically, but that the ophthalmoscope revealed nothing abnormal. The eyes had been removed two hours after death, placed in Müller's solution for two weeks, thence into a solution of gum acaciæ, from that to methylated spirit, which rendered them horny, and ready for imbedding. The gum was removed from the sections by immersion in water, or by simply placing them direct into the staining fluid.

There was a short discussion on the subject of "Finders," and the meeting then resolved itself into a conversazione, when several interesting preparations were exhibited.

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